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The Story of an Observatory

(The Fiftieth Anniversary of the Yerkes Observatory)

By OTTO STRUVE

(Continued from page 244)

I remember one cloudy night, many years ago, when Mr. Van Biesbroeck and I were waiting in our office for the sky to clear. We were talking about the advantages of large reflecting telescopes for many types of research, and decided to look up convenient places where the climate would be more favorable than at Williams Bay. Reference to the Weather Bureau's reports and to the timetables indicated that a location in the Texas Panhandle, near Amarillo, would be within easy reach of Chicago and would give a vastly larger percentage of clear sky than we normally have in the region around Chicago.

At the request of the University administration I prepared two plans of development. I explained that my own work was concerned with the physical study of the stars by means of their spectra and that progress in this field seemed to depend upon being able to record and explain the many different shapes (or contours) of the spectral lines in celestial sources. For this I needed an instrument of great light-gathering power — a 60-inch reflector might do — equipped with an optically perfect spectrograph, capable of recording on photographic emulsions of great contrast (and therefore of low sensitivity) high-dispersion spectra in different wave lengths.

We were then in the worst stages of the great economic depression of the early thirties, and a few attempts on the part of the University authorities to find a donor in Chicago were unavailing. Then, one day, I happened to remember that some years previously a wealthy banker at Paris, Texas, had left his entire fortune of more than $1,000,000 to the State University at Austin for the purpose of constructing and maintaining a large observatory. Mr. W. J. McDonald, it appeared, had always been interested in natural science, including astronomy. In the early days he used to tell his negro barber how some day astronomers would discover with their telescopes the mythical city of golden pavements. Perhaps this thought influenced him in making his bequest.

The University of Texas had no department of astronomy and it was not known whether plans for the observatory were then under consideration. President Hutchins decided to find out. He placed a long-distance call to President H. Y. Benedict at Austin and within a few

minutes the two Presidents had agreed to appoint me director of both observatories, Yerkes at Williams Bay, and McDonald, which then existed only in our imagination.

I went to Austin to discuss the details of the plan. The occasion was a memorable one: it was my first trip by plane, on one of those small six-passenger ships with two rows of three seats, and with the cabin suspended below the wings.

The negotiations between the two universities were quickly concluded: Texas would build the telescope and the entire physical plant and would pay annually a fixed amount towards the operating expenses; Chicago would furnish the entire staff and pay the remainder of the operating expenses. I visited Austin on a beautiful, sunny April day, with the bluebonnets in full glory. Dr. Benedict, incidentally himself trained as an astronomer many years ago at Virginia, took me on a short drive to show me one or two proposed sites for the observatory in the vicinity of Austin. I remember that for once in my life I regretted that the sky was clear: if it had been cloudy it would have been easier to convince the observatory committee that Austin was no place for a large observatory. Fortunately, Dr. Benedict supported my views, as did several other members of the group among whom I remember with gratitude and affection the vice-president, Dr. J. W. Calhoun, and Professors M. B. Porter and J. M. Kuehne. We finally decided that we would equip a small expedition to locate a suitable site, preferably within the borders of Texas, but if necessary in some other state, or even south of the Rio Grande. In the early summer of 1932 C. T. Elvey and T. G. Mehlin were sent in a specially equipped truck to Texas and points beyond in order to obtain some preliminary observations of seeing. Later, I went to Texas myself and chose our present location on what is now called Mount Locke and what then went under the more poetic name U-Up-And-Down Mountain (from the brand of a neighboring ranch).

The many problems which arose in connection with the construction of the 82-inch telescope are so vividly in my mind that I could fill volumes. Our negotiations with the Warner and Swasey Company lasted for nearly a year, with almost weekly conferences in Williams Bay, Cleveland, or Austin. The outcome remained the same: there was not enough money. Then, after we had finally found a solution for the financial problem (by omitting the office building and placing all darkrooms and offices in the dome) there was a change in the State administration at Austin, and one of the new University regents went to Washington and was told by some "well-wisher" that our design which was based upon that of the Victoria 72-inch was completely wrong because supposedly the Victoria telescope had so much flexure that it could not be used at zenith distances greater than 30°.

I must say that despite such minor difficulties my relations with the Regents were always excellent, and I treasure some of them as true friends of the Observatory. The successive presidents of the University, Benedict, Calhoun, Rainey, and Painter have supported every recommendation I have made.

On the last day of 1933, Mrs. Struve, Mr. Elvey, and I were driven over icy roads from a station of the New York Central Railroad to Corning, in order to witness the pouring of the pyrex disc. The operation was a great success and was celebrated with a luncheon in the offices of the Corning Glass Works. But when the annealing oven was opened a few months later we found that the surface was covered with tiny fissures which I undiplomatically described as cracks, but which the glass experts insisted were mere "checks." At any rate the annealing oven was replaced and the entire process repeated. The story was told that in the process of this second melting of the great disc, the mold was slightly expanded through the pressure of the glass, and we finally received an 82-inch disc in place of the 80-inch disc which we had ordered.

The State of Texas had generously promised to build a road to the top of our mountain. On our early trips for testing the seeing we would drive the car up as far as we could and then walk the rest of the way through cacti and over rocks. One of the many difficulties was to secure water. At first we thought of piping the water from a spring about 10 miles away. Later it was decided to drill at the foot of the mountain. Mr. Windy Barnes, a contractor from Ft. Stockton, Texas, drove up one night with Mrs. Barnes who operated a wishing stick and decided upon the exact spot of the well. A few months later I visited the place and found only one unhappy looking workman on the scene. The place which Mrs. Barnes had chosen happened to be above one of those long finger-like rocks which abound in the Davis Mountains, and the bit of the drill would be deflected by the unsymmetrical shape of the rock, causing frequent breakage of the equipment and on that particular occasion even injury to one of the mechanics. But confidence in the wishing stick was still supreme and Windy Barnes refused to drill elsewhere. Fortunately, his contract was void if he failed to find water. So, after having waited for a year or more, we hired a different outfit and obtained an ample supply of excellent water only 50 feet from the place where Mr. Barnes had been drilling.

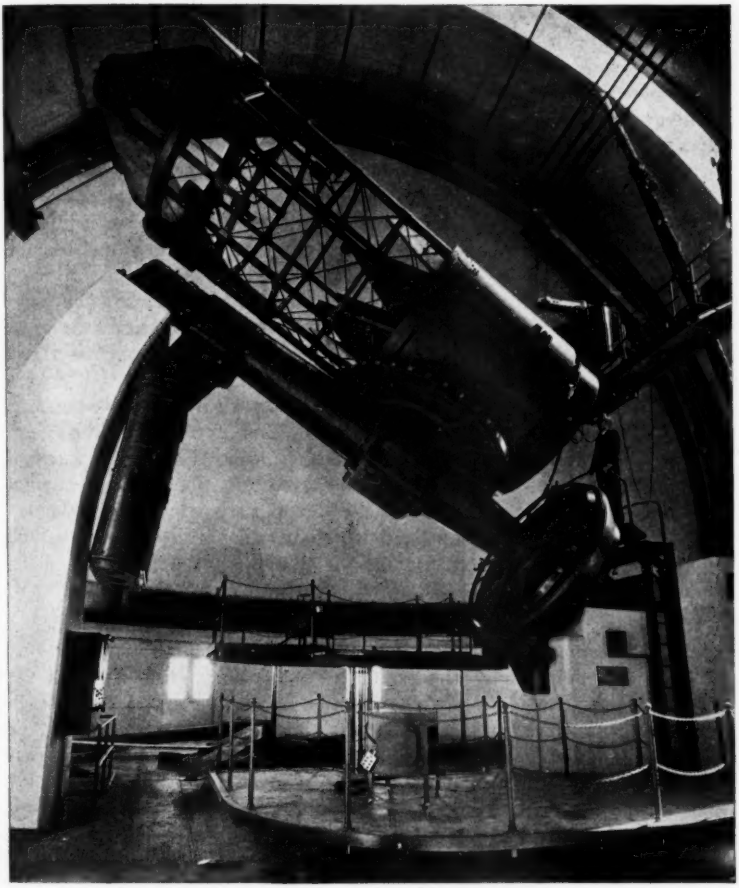
The mechanical construction of piers, dome, and mounting under the able engineering direction of E. P. Burrell of Warner and Swasey made rapid progress. We had transported the old Yerkes 12-inch refractor to Mount Locke and Elvey and I used it extensively as a guiding device for a small f/1 Schmidt camera which a Chicago amateur had made for us and which we were using to record the polarization of the light of several reflection nebulae. We also obtained numerous photographs

of nebulae with red and violet filters and these gave the first indication that the reflecting particles of nebulae have high albedos with scattering functions of the highly forward-throwing type.

While we were working without dome or shelter, we could see the silvery structure of the dome a short distance above us take shape. The parabolizing of the mirror took six weary years. Mr. C. A. R. Lundin, who as a young man had assisted his father and Alvan Clark in the polishing of the Yerkes 40-inch and whose father had also made the famous Pulkovo 30-inch, was employed by Warner and Swasey. He ranks as one of the most expert opticians in the world, and the fine performance of the mirror, when it was tested by Dr. J. S. Plaskett, gave support to this claim. But the years of waiting were nerve-racking. We installed a nebular spectrograph on Mount Locke to while away the many fine nights, and found with it enormous regions in the Milky Way of glowing hydrogen and other interstellar gases. I remember a day in Wisconsin in 1936 when I was driving my friend Bengt Strömgren to Chicago. The idea of finding emission lines of H from interstellar space suddenly came to me as I was telling him about the erroneous work of an Austrian astronomer who thought he had detected emission lines of interstellar calcium. Strömgren took an interest in the idea, and later developed on the basis of our observations a new theory of ionization of interstellar gas which has become the foundation of our knowledge of this subject.

Finally, after many disappointing tests, and after enlisting the assistance of Messrs. Van Biesbroeck, Kuiper, and Pearson, we decided that the mirror was ready. It was "chroluminized" by Robley C. Williams at Cleveland, and was installed at the observatory early in March, 1939. The first night-tests were conducted by Van Biesbroeck and Kuiper. The first scientific result was an ultraviolet spectrum of the expanding shell of 17 Leporis on March 5. A description of this plate appears in the *Astrophysical Journal* for December, 1939. Mr. Kuiper continued observations throughout the remainder of March and April and secured many important results before May 5, the date set for the official dedication. We had several hundred guests and a large scientific colloquim with papers by Milne, Shapley, Hubble, Baade, Trumpler, Bok, Ort, Mrs. Gaposchkin, and many others. The Warner and Swasey Company acted as the host and furnished among other luxuries a rodeo and a complete chuck-wagon dinner for several hundred persons.

On the day of the dedication it was arranged that Mr. C. J. Stilwell, President of the Warner and Swasey Company, would turn over to me officially the keys of the Observatory, while newly elected President Rainey of the University of Texas was to make an appropriate speech. The speech had of course been prepared in advance and was much appreciated. When Mr. Rainey finally came to the words inevitable in most such ceremonies in Texas "and the skies are not cloudy all day,"



THE 82-INCH TELESCOPE OF THE MCDONALD OBSERVATORY

a mild rain began to come from above through the open shutters and I had to close the dome in a hurry.

The construction of the McDonald Observatory had absorbed much more time than I had expected, and at times the optical work seemed to move distinctly in the wrong direction. The clouds of war were gathering all around us. The Lindblads, who had come for the dedication, were still staying at our Williams Bay home prior to their return to Stockholm when the news of the sinking of the "Athenia" was broadcast over the radio; and Dr. A. Unsöld made the return trip to Hamburg on the last scheduled run of the "Bremen." With the exception

of the first few months, the entire life of the McDonald Observatory has been spent in a war-torn world, and the difficulties of organization, of supply, and of staff were exceptionally great. Because of restrictions in rubber and gasoline we had to put up new residences for staff members who had previously lived in town. We had to build several electrical installations in order to safeguard ourselves against temporary breakdowns: repairs to motors, etc., were practically impossible to secure. Of the non-academic employees only one engineer was given occupational deferment from selective service. The rest were drafted, one after another, so that the turnover of our service personnel became so rapid that the offices in Chicago had difficulty in keeping up with their names. At many occasions during the war we felt near the point of closing the institution. My own attitude to this question was quite clear: the Observatory represented a valuable new cultural asset and every effort must be made to preserve it and to keep it going. We were training physical scientists who were being absorbed in the tremendous scientific war effort of the United States, but even more important, though at the time not appreciated, was the idea that after the war had been ended the country would need all the centers of quiet fundamental research it could preserve. Hence, I never wavered: in every emergency all escapes had to be tried which would permit us to continue operations. It was a satisfaction to me that we lost not a single hour of observing due to causes connected with the war.

A few years after my appointment as director (about in 1935) I attended an annual dinner given by the Trustees of the University of Chicago for members of the faculty. As I was passing through the receiving line President Hutchins came up to me and asked me to visit him soon at his office. When I did so, a few weeks later, he asked me whether I had given some thought to the question of increasing and improving the staff of the astronomy department and he outlined to me his own policy of building up weak departments through the appointment of only relatively young first-class research workers. Were there such workers available in astronomy? I replied that I could name three or four immediately: Kuiper, Chandrasekhar, Bengt Strömgren, and one or two others. Mr. Hutchins questioned the wisdom of inviting a mature man of 40, expressing a definite preference for men of about 35, or younger. This was the beginning of a new policy of staff-building which has been at the bottom of most of our organizational changes. I continued going to the President's office and discussing with Mr. Hutchins various questions of personnel. His attitude was always favorable if I could describe a candidate as the best worker in his field. Really good men are so difficult to find that Mr. Hutchins invariably offered to appoint those who merited the designation, irrespective of whether or not their work fitted into the established program of the department; if it did not fit, then the program had to be

modified. Questions of salary, equipment, etc., were also secondary in his mind.

Mr. Hale has described the simple scientific program which formed the basis for the Yerkes Observatory. My own scientific program was equally simple. Upon my arrival in Williams Bay in 1921 Professor Frost had assigned to me the measurement and reduction of thousands of spectrograms obtained with the Bruce spectrograph for the determination of the radial velocities of the A- and B-type stars. My studies in Russia had been interrupted by four years of military service in World War I, and in consequence I had had only a superficial course in astrophysics (under the late B. P. Gerasimovič). The only kind of spectrum I had an acquaintance with was that of the sun as seen through a small prism spectroscope. My impression of the Fraunhofer absorption lines was of countless narrow and sharply-defined black lines whose shapes or contours had no interest because they were more or less faithful images of the shape of the slit. I was greatly disappointed when I found that more than one-half of the stars I was to measure had a few hazy, broad, and extremely ill-defined features in place of lines, and for some time it was difficult for me to understand how one could measure such features at all. Since then my principal aim has been to explain the great variety of contours of stellar absorption and emission lines, and in a very large measure progress in this field has been an accomplishment of the Yerkes Observatory. Problems of stellar rotation, the Stark effect, turbulence, and collisional broadening one after the other held the spotlight. It became apparent that ordinary photographic emulsions did not possess enough contrast to bring out the slight shadings and differences in nuance between different lines, so that the slow Eastman Process emulsion had to be employed despite a factor of almost 20 in the exposure times. This made it impossible at Yerkes to observe with adequate dispersion any but the very brightest stars. A large reflector was urgently needed, and the construction of the 82-inch at McDonald gave me adequate light-gathering power for all those problems in stellar spectroscopy which I had contemplated. I have estimated that with the 82-inch reflector I quite regularly secure in one month of concentrated work a larger amount of observational material, and material of far better quality, than I had secured with the 40-inch in 20 years of work!

The completion of the McDonald Observatory marked the fulfillment of the first task I had set myself in 1932. The other task, equally important and in some respects more difficult, was to reorganize the staff of the department. When I was appointed director the reputation of the institution was upheld by Mr. Van Biesbroeck and Mr. Ross. The addition of Chandrasekhar to the staff gave us our first strong representation in the field of theoretical astrophysics. His influence has expanded enormously during the past ten years, and his recent promotion

to a distinguished service professorship in the University of Chicago serves to underline the importance which his work is accorded in our present organization.

The formulation of a plan of research is perhaps the most difficult task of an observatory administrator. In our case this plan was not entirely free; it was constrained by two outside conditions: we were committed to Mr. Hutchin's program of faculty-building, and we were limited in our equipment of instruments of the type of the 82-inch reflector and the 40-inch refractor.

Very broadly, our research plan falls into four categories:

I. *The study of the physical properties of the outer layers of the stars.* This includes much of Chandrasekhar's work on radiative transport, on continuous absorption, on polarization, and on moving atmospheres. It also includes most of my own work on line contours, as well as joint studies with Kuiper and Strömgren of such problems as those of ϵ Aurigae and β Lyrae. Professor Herzberg's spectroscopic laboratory is the realization of one of Mr. Hale's principal wishes in this field.

II. *The study of the stars as building blocks of our galaxy.* This includes, first of all, Kuiper's work on the spectra of faint proper-motion stars, Morgan's work on spectral classification, and Van Biesbroeck's recent studies of faint companions of known proper-motion stars. It also includes all of our work on parallaxes, mass ratios, visual double stars, spectroscopic binaries, as well as Popper's studies of the populations of globular clusters. Chandrasekhar's work on the origin of the chemical elements and Kuiper's investigations of the hydrogen content in clusters serve as theoretical guides.

III. *Problems of gaseous nebulae and interstellar matter.* Our principal contributions to this field were made with the nebular spectrograph at McDonald. More recently the work of Swings on peculiar emission-line stars should be entered in this classification and also Greenstein's studies of the spectrum of the Orion nebula, of the ratio of galactic absorption to reddening, etc. The theoretical work of Henyey and Greenstein and the observational work of Elvey on the galactic light and on reflection nebulae formed an important contribution.

IV. *Miscellaneous studies of the sun, planets, comets, etc.* Best known in this field are Kuiper's studies of atmospheric absorption on different planets and their satellites, as well as his recent infrared studies of the planets. Of interest are also Van Biesbroeck's observations of the tremendous fluctuations in the brightness of comet Schwassmann-Wachmann, etc.

It will be immediately noticed that the Yerkes Observatory has almost completely disappeared from the field of solar research. There have been sporadic efforts to revive it, especially by Pettit and by Keenan, but, stimulating as their results were, they have not been followed up.

In this respect we have not lived up to Mr. Hale's original expectations. But in most other respects we have the satisfaction of seeing his plans realized, and his confidence rewarded. We have followed him in his preference for large telescopes, and have added to the largest refractor the second largest reflector. After a long interruption we recognized the wisdom of combining a spectroscopic laboratory with the observatory, and we confidently expect that the collaboration between Mr. Herzberg and the telescopic observers will fully confirm Mr. Hale's views. We also followed him when we brought Fred Pearson to Yerkes and started building our own accessory optical instruments.

In the past few years there has been a large change in astronomical planning. The scope of our science has increased enormously. The spectral range has been extended on the violet side through the use of the German V-2 rockets and on the red side through the new PbS photoelectric cells, and especially through the microwave and radio-wave techniques. Electronics has become an important tool. It is clear that we must reorient ourselves to take account of these changes. A new plan must be devised and a new policy must be adopted if the Yerkes Observatory is to retain its place among the leading research centers of the United States. The construction of a new large telescope, such as I had considered some years ago, may no longer be the most important problem that confronts us. Research in electronics may become our principal task.

It may also be doubted whether the routine process of building large telescopes on conventional lines is best suited to our department. The image produced by a large mirror is a complicated structure which appears sometimes seriously distorted by effects of "bad seeing" in the tube and which has a boiling or flaming appearance. The only reason why Mr. Kuiper and Mr. Van Biesbroeck are able to measure double stars at McDonald to within the theoretical limit imposed by the diffraction pattern is that small condensations within the large boiling image have the theoretical diameter, while a large amount of the "boiling" light is spread over a disc which even under good seeing conditions is rarely much smaller than 1 second of arc in diameter. A careful study of the conditions that are required for the formation of good images, such as has been conducted, more or less sporadically, by Baade at Mount Wilson and by Lyot in France, may well result in the same kind of progress as that which enabled Lyot to produce a coronagraph and with it to solve Hale's old problem of observing the corona without an eclipse. Hale's failure to photograph the corona in full sunlight was undoubtedly the greatest disappointment of his scientific life. He tried it on Mount Aetna and at Williams Bay, at Mount Wilson and at several other sites—but always without success. Yet, the method that is now being employed by Lyot and his successors is not very different, in principle, from the one used by Hale. Only greater precautions

against scattered light by dust on the surfaces, and by small bubbles or scratches and other defects in the optical parts, were required to insure complete success. Is it not possible that a thorough study of image-formation of a large mirror would produce an unexpected gain in light-concentration?

Astronomy is at a threshhold. If we shall continue along the old lines laid out 50 years ago, or even only a short 15 years ago, we shall rapidly lose our place in science.

After careful consideration of the many questions involved I decided, a few months ago, to ask the University of Chicago to relieve me of the responsibility of the directorship of the two Observatories. This is an important and a somewhat unusual step to take, and I want to explain it in detail.

YERKES OBSERVATORY STAFF, MAY, 1946

First Row: Left to right—Dorothy Deutsch, Martha Carlson, Anne Underhill, Guido Münch, Doris Blakeley, Margaret K. Krogdahl, Nancy Roman, Carlton Pearson.

Second Row: P. Swings, G. Herzberg, Mrs. G. Herzberg, W. W. Morgan, O. Struve, J. L. Greenstein, G. P. Kuiper, S. Chandrasekhar, G. Van Biesbroeck, L. G. Henyey.

Third Row: Paul Ledoux, Margaret Phillips, John G. Phillips, Roy Wickham, W. P. Bidelman, Frances H. Breen, Alice Johnson, Marguerite Van Biesbroeck, Edith M. Janssen, John Vosatka, H. Bernstein.

Fourth Row: Arne Slettebak, Fred Pearson, Armin Deutsch, Merle Tuberg, Marshal Wrubel, Irene Hansen, Arthur Code, Gertrude Peterson, Carlos U. Cesco, Victor Blanco.

I believe that I have made it clear that the principal tasks which I had set myself 15 years ago have been carried out. The 82-inch reflector is working beautifully, the staff of our organization is as strong as it could be, and my own previous research interests in stellar spectroscopy have been satisfied. I have also explained to you why I think that a complete reorganization and reorientation of the observatory is in order. The administrative work of the department has

increased many fold and the practical methods which I developed to cope with it are no longer adequate.

The changes in personnel which we made almost 15 years ago have borne fruit and the men who came to us then as promising beginners have become recognized authorities in their respective fields. We are fortunate that some of them have high administrative ability. To fail to make use of this ability would deprive us of an important advantage. It is my opinion that now is the right time to turn the administrative duties over to another man. I am glad to announce that the University has acted favorably upon my recommendation and has agreed to appoint Mr. Kuiper director of the two Observatories. Mr. Hiltner will continue as assistant director.

The department of astronomy of the University of Chicago will consist of four essentially independent branches: the two Observatories under Kuiper, a section on theoretical astrophysics under Chandrasekhar, the *Astrophysical Journal* under the managing editorship of Morgan, and the teaching section in Chicago. I shall remain chairman of the whole department and the four persons in charge of their separate sections will be responsible to me. But it is not my intention to occupy myself with the details of management. Instead, I shall devote my time to the study and development of plans of long range—a task which in the past has not received proper attention. I shall also be responsible for preparing the combined budget of the department and I expect to assist in working out a smoothly functioning system of administration. In this manner Chandrasekhar would not only become the head of a recognized group of research workers who would no longer be expected to necessarily take part in the observational work, but he would also become responsible for the graduate teaching at Yerkes, a task which he has informally (and without proper recognition) been carrying for several years.

I feel extremely enthusiastic about the reorganization which I have proposed. Its acceptance by the University, at a considerably increased cost, is a sign of the continued support which the administration is prepared to give. The recognition that is implied in Mr. Kuiper's appointment, and also in the other new appointments, should act as a strong stimulus and should bring about an impulse that will benefit our entire organization.

I am leaving the directorship with the feeling that the past 15 years have given me a wonderful opportunity to build up a fine observatory with an excellent staff. The experience has been one I should not like to have missed. I considered it a real challenge when I was called upon to modernize the scientific work here and to plan and build the McDonald telescope. In turning over my principal administrative duties to Mr. Kuiper I wish to give expression to my feeling of complete con-

fidence in his great ability and unusual versatility as an astronomer, in his sound administrative views, and in the forcefulness of his character. The observatories will be in good hands!

A Sinuous Photographic Meteor Trail

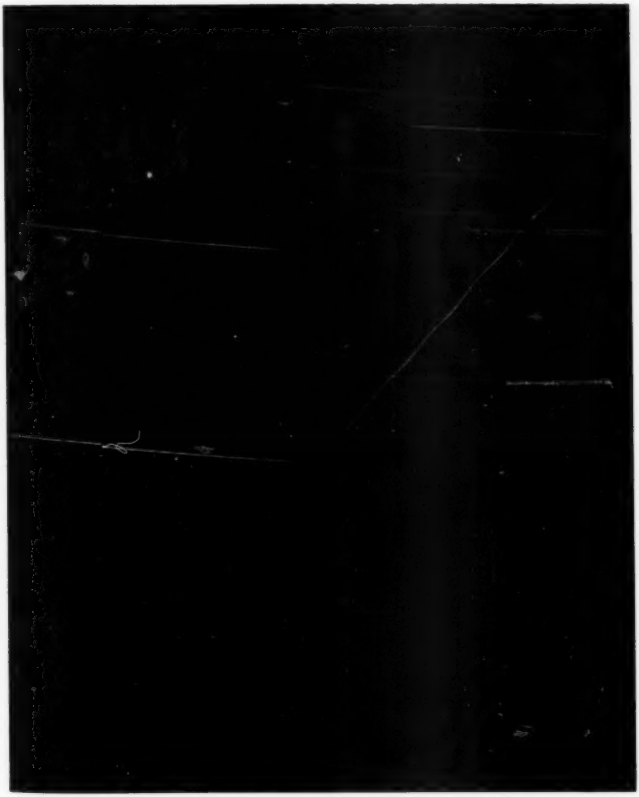
By CARL AUGUST BAUER

1. Introduction. It is not unusual to hear reports of visual observations of meteors showing conspicuous undulations in their motion through the atmosphere. However, the many photographs of meteor trails which have been carefully measured never show sinuous trails representing real undulations in the motion of the meteors and, moreover, these photographs rarely show curvature of any kind to a degree that could be observed visually. In the Harvard collection of meteor photographs there are some trails which upon close examination show sinuosity of very small amplitudes. Dr. Fred L. Whipple[1] has shown that this sinuosity was produced by camera vibrations induced by the driving mechanism.

There have been frequent and reliable visual observations of meteors moving in paths curved moderately in one direction and occasionally slightly curved trails occur on photographs so that there is no reason to doubt the reality of such motions. However, the occurrence of undulations, spirals, or abrupt curves in the motion of meteors, while reported from time to time for visual observations, has never been recorded photographically. Motions of this kind require large lateral accelerations and thus differ, in this respect, from the motions in paths curved moderately in one direction. Since curved trails requiring large lateral accelerations have not been photographed, and since they appear impossible from physical considerations, and since a visual impression of curvature may be caused by splitting, light fluctuations, or second independent meteors, it is necessary to regard visual reports of such motions with suspicion. Nevertheless, it is important to examine all reports of such motions. If sinuous or abruptly curved meteor trails appear on photographs it is important that the curvature be adequately explained in each case.

2. Observational Material. During the October 9, 1946, Giacobinid meteor shower many meteors were photographed with cameras mounted on fixed supports and thus giving trailed star images. Reports have been received that a few of these photographic trails show remarkable curvature. One of the negatives received at Harvard shows a trail which exhibits 12 cycles of a sinuous curve. It was considered advisable to make a careful analysis of this negative because there has been speculation as to whether this curve represents a real curved motion of the meteor through the atmosphere, and because of the importance of the

possible existence of such a phenomenon. This negative was secured by the members of the Jacksonville Amateur Astronomers Club and was sent in by E. L. Rowland, Jr., of Jacksonville, Florida. According to Mr. Rowland, "The picture was made with a Schneider-Krueznach lens of 5.5 diameter and a focal length of 36 cm, mounted on a 4 × 5 Speed Graphic camera with a Compound shutter. The camera was mounted on a heavy motion picture tripod. There was no wind. The film used was fresh Eastman Super-Pan Press Type B. The exposure was 20 minutes from 9:30 to 9:50 P.M. (E.S.T.). The field covered was approximately 15 by 20 degrees."



THE SINUOUS PHOTOGRAPHIC METEOR TRAIL

The trailed star images show two breaks. The first break occurs when 0.42 of the total exposure time has elapsed. The second break occurs when 0.52 of the total exposure time has elapsed and at this

break there is a lateral displacement of about 0.2 mm. According to Mr. Rowland, "The break in the trail of the stars in the picture, we found, apparently was caused by the operator of the camera, who, observing that he had caught a picture of a meteor, inserted the slide, then decided to further expose the film and removed the slide for additional time." It seems somewhat doubtful whether the insertion of the slide should be associated with the first or second break.

The meteor trail and the star images were carefully examined with a 20-power binocular microscope. For the bright star Deneb this examination revealed a faint fine curved line at the beginning of the trail and at the second break. In both cases this fine line describes a few somewhat irregular ellipses and then merges into the ordinary trailed star image. This fine line can not be followed in sufficient detail but it appears that it is continuous across the second break. This evidence indicates that the dark slide was not inserted at the second break but that the camera was disturbed in some other manner. Measurements of the ellipses defined by this line showed that their size and orientation are about the same at the beginning of the trail and at the second break.

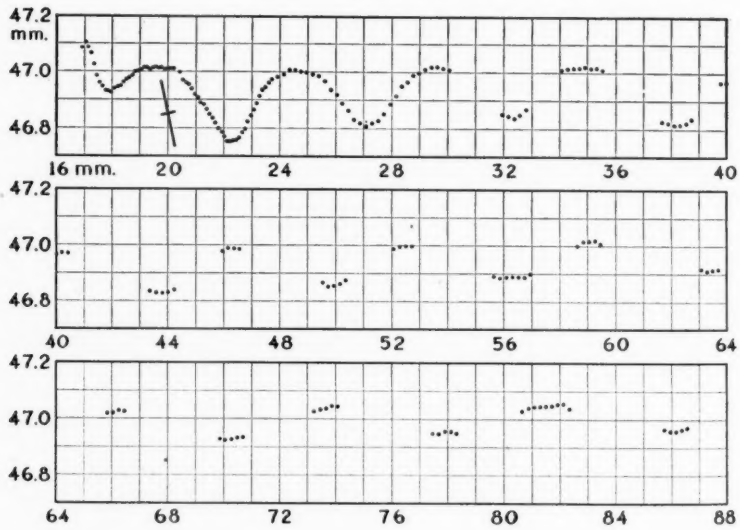


FIGURE 1
MEASURES OF THE SINUOUS METEOR TRAIL

In plotting, the scale of the ordinate has been extended by a factor of 10 relative to the scale of the abscissa. For convenience, the orientation and axes of the ellipse indicated by the trail of Deneb are shown by the cross near the beginning of the plot. The orientation and relative length of the axes of the ellipse are distorted by the extension of the scale of the ordinate.

The appearance of this line shows that the camera vibrated with an elliptical motion both at the beginning of the exposure and at the second break. The faintness of the lines and the appearance of the ellipses indicate that the vibrations were rapid and quickly damped. These lines can be traced in some detail only for the bright star Deneb though their occurrence can just be detected for one or two other bright stars. It is fortunate that the field included a very bright star, as otherwise the occurrence and character of the vibrations would have been much more difficult to establish.

3. Measurements and Reductions. The x and y coordinates of points along the meteor trail were measured with a measuring engine. The negative was adjusted so that the x-axis was closely parallel to the motion of the meteor with x increasing in the direction in which the meteor moved. Numerous settings were made through the first three cycles and several settings near each of the subsequent maxima and minima. Figure 1 represents these measures graphically. The position of the center of the film and the position of four stars were also measured.

Figure 2 shows the amplitude of the oscillation in the meteor trail plotted for each cycle.

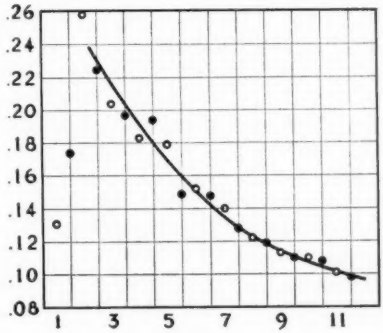


Figure 2
The Amplitudes of the Oscillations

The ordinate represents the amplitude in mm. The abscissa represents the cycle of the oscillations, starting from the beginning of the trail.

Figure 3 shows the relative distance traveled by the meteor during each cycle. The absolute distance traveled by the meteor during each cycle can be obtained by multiplying the ordinates in Figure 3 by D, the distance (about 97 km) from the camera to the beginning point of the meteor trail. Finally, the period of each cycle can be found by dividing the above absolute distance by the linear velocity (23 km/sec) of the meteor. It is evident that the meteor travels very nearly the same linear

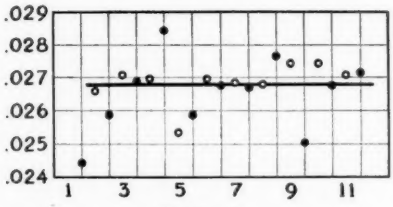


FIGURE 3

THE RELATIVE DISTANCES TRAVELED BY THE METEOR IN SUCCESSIVE CYCLES

The abscissa represents the cycles. The ordinate represents the ratio s/D, where s is the distance traveled by the meteor in one cycle and D is the distance of the beginning point from the camera. Assuming D = 97 km and the velocity = 23 km/sec, the mean period, corresponding to the horizontal line, is 0.113 sec, and 0.001 on the ordinate corresponds to .004 sec.

distance during each cycle. In constructing Figure 3 it was necessary to take into account the orientation of the radiant, trail, and camera and the change in scale from the center to the edge of the film.

4. LATERAL ACCELERATIONS. With the history of camera vibrations revealed in the star trails it appears very probable that these vibrations account for the undulations in the meteor trail. However, this explanation of the shape of the trail requires that the meteor appeared during one of the two very short intervals while the camera was vibrating. The occurrence of the meteor *immediately after* the dark slide was drawn does not agree with the impression of the observers. Therefore, it seems important to consider the consequences that follow from the interpretation of the trail as representing a real curved motion of the meteor.

If the meteor is assumed to have traveled in a spiral or a plane sinuous path the acceleration of the particle toward the center of its circular component of motion can be calculated. Since the Giacobinid meteors are known to strike the atmosphere with a velocity of about 23 km/sec the height of the beginning point of the trail can be taken as approximately 85 km according to the data of Dr. Fred L. Whipple.[2] The altitude above the horizon of the beginning point was 61° 44′ and thus the beginning point was about 97 km and the middle of the third cycle about 95 km from the camera. Since the third cycle is typical of the oscillations and appears relatively undisturbed, it is used in the following sample calculations. The amplitude of the third cycle is about 0.1 mm = 1′. Thus the linear amplitude, *i.e.*, the radius of the circular component of the assumed motion, is r = 0.028 km. The meteor travels 2.55 km during the third cycle and thus the third cycle is described in t = 0.111 second. For the acceleration toward the center we have,

$$a = V^2/r = 4\pi^2 r/t^2 = 90 \text{ km/sec}^2.$$

The lateral acceleration required to produce the observed trail is thus about 10^4 times the acceleration of gravity and about 10^2 times the

measured decelerations of meteors in the forward direction due to the resistance of the earth's atmosphere. The decelerations[2] are usually of the order of 1 km/sec^2 and are very rarely as high as 10 km/sec^2 except near the ends of trails. The lateral accelerations that have previously been determined for photographic meteors have amounted to several times the acceleration of gravity. Since the lateral acceleration of a meteor must be caused by the interaction of the atmosphere with a particle of irregular shape or structure by processes similar to those which resist the forward motion of the meteor, it appears that the value of the deceleration of the meteor in the forward direction is a high upper limit to the lateral accelerations and that ordinarily lateral accelerations should be expected to be less than 100 g.

Consequently, the interpretation of this sinuous meteor trail as representing a real curved motion of the meteor is considered erroneous because: (1) this interpretation requires extremely large lateral accelerations. In addition, (2) this interpretation requires that larger lateral accelerations occur near the beginning of the trail when the meteor is at a greater height and thus in less dense air than it is at the end of the trail. Such motion would not be expected from any of the processes proposed to account for lateral accelerations. Note that this effect would be even larger than indicated by Figure 1 or Figure 2 since a correction for the distance of each cycle from the observer would need to be applied to the angular amplitude in order to obtain the linear amplitude and thus the acceleration. (3) This interpretation requires that the meteor persisted in the same type of relatively uniform oscillations through 12 cycles while it was constantly changing its shape and mass due to its disintegration in the earth's atmosphere. Consequently, even if there were an absence of another explanation for the shape of the trail the sinuous trail could not be interpreted as representing the true motion of the meteor.

It is interesting to consider the possibility of observing meteors moving in sinuous paths through the atmosphere. If we consider as a favorable case a meteor at a distance of 50 km from the observer and suppose that it has, in addition to its forward motion, a circular component which is described in 0.1 second, and if we allow the lateral acceleration to reach the very large value of 10 km/sec,[2] then the radius of curvature of the circular component of its motion is only 0.0025 km. The total range of the undulations would then subtend an angle of only 10″ which certainly could not be observed with the naked eye. Thus the present discussion makes it appear extremely unlikely that meteors can travel in spiral or undulating paths through the atmosphere with amplitudes of the sinuous motion sufficiently large to be observable with the naked eye or on small-scale photographic plates. The reports of visual observations of such paths may be attributed to the occurrence of more than one meteor at about the same place and

time, to the splitting of single meteors, to flares, or to exaggerations of moderate curvature. The present discussion does not exclude the visual observability of paths showing moderate curvature.

5. Camera Vibrations. Notwithstanding the impression of the operator of the camera and the requirement that the meteor appeared during one of the very short intervals while the camera was vibrating, it appears that we are compelled to attribute the sinuosity of the meteor trail to camera vibrations. The elliptical trails of Deneb and the decrease in the amplitude of the oscillations in the meteor trail indicate that the camera vibrated for about 2 seconds both at the beginning of the trail and again at the second break. It is interesting to see to what extent the shape and character of the trail can be accounted for by the existence of elliptical vibrations of the orientation and amplitude indicated by the trail of Deneb. For convenience the axes of the ellipse indicated by the trail of Deneb are shown in Figure 1. We can consider the elliptical vibration as the resultant of two damped linear harmonic motions of different amplitudes vibrating at right angles to each other with the same period. The following arguments show that these vibrations fully account for the appearance of the meteor trail.

(1) Figure 2 shows the amplitude of the oscillations in the meteor trail. It is seen that, neglecting a few irregularities, especially near the beginning of the trail, the amplitude follows an approximately exponential decrease with time as is expected for damped vibrations. The few irregularities are not unexpected since similar irregularities occur for the trail of Deneb. The amplitudes of the vibration can be accounted for by elliptical vibrations of the size and orientation indicated by the elliptical trails of Deneb.

(2) Since the period of damped vibrations remains constant it is important to note how Figure 3 shows that the relative linear distance traveled by the meteor during one cycle of the vibration remains nearly constant except for a few irregularities especially near the beginning of the trail. When the unexpected regularity in the points in Figure 3 is considered the absence of a negative slope of the straight line indicates that the deceleration of the meteor must have been small (probably less than 1 km/sec^2).

(3) The trail does not have exactly the shape of a sine curve in that the minima are narrower than the maxima and also the minima are reached a little past the points midway between the maxima. Such a shape is fully accounted for by the resultant of the linear motion of the meteor and the elliptical vibrations of the camera.

(4) The meteor trail is denser at the minima, especially those minima near the beginning of the trail when the vibrations are greatest. This appearance is also fully accounted for by the resultant of the two motions. At the denser points in the trail the motions of the meteor

image and the plate were in the same direction, so that the relative speed of the image across the plate was less at these positions, and thus the exposure was effectively longer than for the less dense broad maxima where the motions of the meteor image and the plate were in opposite directions.

(5) For several cycles near the middle of the trail where the meteor was the brightest, the density of the trail fades off gradually in the direction from which the meteor came. This appearance is attributed to the train of hot luminous gases in the wake of the meteor. Because of the component of vibrational motion at right angles to the direction of motion of the meteor the images at the nodes of the sinuous curve are effectively an instantaneous photograph of the meteor and the luminous gases in its wake. This appearance is not compatible with a real sinuous motion of the meteor, for in this case the luminous gases would be left along the path that the particle traversed. This appearance emphasizes that we should not consider a meteor as a moving point source of light but that we must take into account the light from the luminous gases in its wake. This phenomenon is an important limiting factor in the design of rotating shutters for meteor cameras.

I wish to express my appreciation to Mr. E. L. Rowland and the Jacksonville Amateur Astronomers Club for the loan of their negative and to Dr. Fred L. Whipple for valuable discussions of this problem.

REFERENCES

[1] *Proc. Am. Phil. Soc.*, **82**, 275, 1940.
[2] *Rev. Mod. Phys.*, **15**, 246, 1943.

John Couch Adams and the Discovery of Neptune*

By W. M. SMART†

The discovery of the planet Neptune, one hundred years ago, was the result of very unusual and dramatic circumstances. Since the discovery practically every writer on astronomy has set forth the principal facts in connection with it. However, the recognition of the centennial last year brought out some material not generally known before. Three papers bearing on the question were published in NATURE. *We are privileged to reprint these papers here for the benefit of those who may not have access to the copies of* NATURE. *We do so with great satisfaction.* EDITOR.

Until 1781, the planet Saturn represented the outermost boundary

*Summary of addresses to the Royal Astronomical Society on October 8 on the occasion of the centenary celebrations of the discovery of Neptune, published in *Nature*, November 9, 1946.

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of the solar system; on March 13 of that year the planet Uranus was discovered by Sir William Herschel, and by the beginning of 1846 (the year of the discovery of Neptune) five minor planets had been found. In all these instances, the discovery was made at the telescope, in one or two cases purely by accident. The discovery of Neptune was on a far different level of human achievement; the discrepancies between the predicted and observed positions of Uranus since its discovery furnished the means whereby two mathematicians, Adams and Le Verrier, applied their unrivalled skill to deduce independently the position of a new planet the gravitational attraction of which on Uranus, they confidently believed, was responsible for the discrepancies referred to.

When, shortly after 1781, an approximate orbit had been calculated for Uranus, it was suggested by Bode that perhaps the planet had been observed previously as a 'star'; the search of catalogues proved surprisingly successful, for no fewer than nineteen authentic observations of Uranus had been recorded, the earliest in 1690 by Flamsteed (the first Astronomer Royal), who designated it 34 Tauri. In the nomenclature of the time, these pre-discovery observations of Uranus are known as the 'ancient' observations, those after discovery as the 'modern' observations. In the second decade of last century the accurate establishment of the planet's orbit was undertaken by Bouvard, who was soon faced by a peculiar difficulty. If he used the 'ancient' observations alone, he obtained an orbit differing unmistakably from the orbit derived from the 'modern' observations alone, these covering nearly forty years. In this dilemma he rejected the 'ancient' observations entirely, on the plea that they carried very much greater observational errors than the 'modern' observations, and his tables of Uranus, published in 1821, were based entirely on the latter. But soon Uranus was seen to be falling behind its predicted position; by 1832 the error in longitude was ½′, and in 1837 Airy (the Astronomer Royal) reported that the errors were "increasing with fearful rapidity"; the anomalous behaviour of the planet had now become the most puzzling problem in contemporary astronomy.

Several suggestions were offered to account for the phenomenon; the law of gravitation might not be exactly according to the inverse square of the distance (a suggestion regarded by Airy as possible even so late as 1844); the existence of a resisting medium—an ever-popular hypothesis—was put forward; perhaps the errors in the positions of Uranus were due to a massive satellite (how this could have escaped observation was not stated); perhaps about the time of discovery in 1781 Uranus had been hit by a comet, this suggestion being made, of course, to explain the difference in the orbits derived separately from the 'ancient' and 'modern' observations; and finally it was hazarded that the discrepancies—or, technically, the perturbations—resulted from the attraction of an undiscovered planet far beyond the bounds of Uranus.

Airy himself had no doubts about the last hypothesis, for he wrote: "If it (the anomalous behaviour of Uranus) be the effect of any unseen body it will be very nearly impossible ever to find out its place." Fortunately, Adams and Le Verrier had a clearer perception of the problem than Airy, and they were little daunted by the difficulty and magnitude of the task to which in due course they applied their incomparable mathematical skill. From the beginning they were supremely confident of the existence of an unknown planet and of the power of analysis to ensure its optical discovery.

When Adams was still an undergraduate at St. John's College, Cambridge, he and a companion, Drew, were discussing their futures. When Drew asked him what he proposed to do, Adams replied deliberately: "You see, Uranus is a long way out of his course. I mean to find out why. I think I know." Drew said afterwards that this reply gave him a queer feeling, as if a young prophet were speaking. Adams was born on June 5, 1819, the eldest of the seven children of a Cornish farmer. In 1836 his mother inherited a small property, and it seems almost certain that but for this 'windfall' the family economy—always exiguous—would never have stood the strain of a university education for the future astronomer. In January, 1843, Adams was Senior Wrangler, and within a few months he had won the First Smith's Prize and had been elected to a College fellowship. Earlier, on July 3, 1841, Adams wrote his celebrated memorandum—now preserved in St. John's College library—in which he expressed his determination to start operations, as soon as he had taken his degree, on the mathematical discovery of a trans-Uranian planet.

The problem to which Adams devoted his energies during 1843-46 was one of considerable complexity. On the hypothesis of an unseen planet, the orbital elements of Uranus as deduced by Bouvard must be somewhat erroneous, for the observed positions of Uranus which he used must be affected by perturbations of which he was unaware; the corrections to Bouvard's orbital elements of Uranus constituted the first group of the unknowns in the mathematical formulation of the problem; to these must be added the mass of the hypothetical planet and the elements of its orbit. Owing to the way in which the mean distance of the new planet entered into the equations of condition it was necessary, if the problem were to be made practicable, to assume some value for this mean distance.

Adams, and afterwards Le Verrier, started with the value suggested by Bode's Rule as applied to the known planets. Before the end of 1843, Adams—he was then only twenty-four—had arived at a preliminary solution which convinced him that the hypothesis of an unknown planet was adequate to explain the anomalous behaviour of Uranus. He then proceeded to introduce some necessary refinements into his mathematical investigations. By September, 1845, he had made such progress

that he was advised by Challis—then Plumian professor and director of the Cambridge Observatory—to place his results before Airy. Accordingly, when on his way to Cornwall for a holiday in September, Adams, armed with an introduction from Challis, called at the Royal Observatory, Greenwich, only to discover that Airy was in France. On his return from Cornwall, Adams again called on Airy (October 21, 1845); Airy was out at the time, but Adams left his card and a message to say that he would call in about an hour; he did so, but was informed that Airy was at dinner. Adams had perforce to depart, leaving, however, for the Astronomer Royal a "short statement" of the results of his researches, which, as we now know, were adequate to ensure the optical discovery of the planet at that time. Airy wrote to Adams fifteen days later putting his famous question as to whether Adams' theory could also explain the discrepancies between the values of the radius vector, as computed on Bouvard's theory, and the values which Airy had derived from observation. Adams did not bother to reply; he was not prepared to regard the question as other than trivial (although at the time he was only twenty-six, he was a master of planetary theory), and further he was disappointed that his efforts to make personal contact with Airy had proved fruitless. Adams' "short statement" remained in Airy's pocket for eight months, and probably would never have seen the light of day if events in France had not rescued it from oblivion.

In November, 1845, Le Verrier read his first memoir on Uranus; this can be described simply as "Bouvard" amended and brought up to date; there was no mention of a hypothetical planet. In June, 1846, Le Verrier read his second memoir, in which, after discussing the reasons for the necessity to assume the existence of an extraneous planet, he announced the position of a hypothetical body as deduced from his mathematical investigation; the mass and the elements of the orbit were not stated. The position obtained by Le Verrier was within a degree of the position found by Adams.

Towards the end of June, 1846, Airy put the same query about the radius vector to Le Verrier as he had put to Adams eight months earlier. Le Verrier replied without delay, assuring the Astronomer Royal that his theory accounted automatically for the errors in the radius vector; further, he applied to Airy for assistance in the search for the planet, promising to send him at once fuller details of his work. This request for practical aid and the offer of more precise information passed unheeded; nor did Airy inform Le Verrier that mathematical investigations of a similar character had been in progress at Cambridge for nearly three years previously. A day before Le Verrier's letter reached Airy, the latter announced to the Board of Visitors of the Royal Observatory the almost identical results—as regards the longitude of the new planet—obtained by Adams and Le Verrier, and on July 9, realizing that the situation was indeed becoming "desperate"—as he

described it—he wrote to Challis, the director of the Cambridge Observatory, asking him to undertake the search for the new planet with the Northumberland Telescope, at that time one of the biggest instruments in the world. Challis agreed, and the search began on July 29. In the absence of a stellar chart of that part of the sky in which the planet was believed to be situated, Challis had perforce to undertake a laborious program of observations, determining the positions of all the stars within the suspected zone. Up to the end of September, when the news of the telescopic discovery of Neptune at Berlin reached Cambridge, Challis had made altogether 3,150 observations of stars and, as it transpired afterwards, had actually observed the planet on four occasions.

On August 31, 1846, Le Verrier's third paper was presented to the Academy at Paris; in this paper he gave the mass and the orbital elements of the planet and also stated that the planet should show a disk of about 3″ in diameter, which observations in due course confirmed almost exactly. Two days later Adams wrote to the Astronomer Royal giving him the results of a new solution of the problem, and, remembering Airy's former query about the radius vector, he indicated how reasonably well his theory fitted in numerically with the established errors of radius vector. There can be no doubt that, at this time, Adams was entirely ignorant that Le Verrier was hard on his heels; it is also certain that Le Verrier had no inkling of Adams' investigations.

On September 18, Le Verrier wrote to Galle, the assistant at the Berlin Observatory, requesting the latter to undertake the search for the planet; the letter was received on September 23, and Galle decided to start operations at once. A young student-observer, d'Arrest, suggested that the first thing to do would be to find out if Bremiker's star-chart (Hora XXI)—which included the zone in which the planet might be expected to be found—had been finished. A search in the director's house proved successful. There they found the edition of the relevant chart which had been engraved at the beginning of 1846 and which was being held back from distribution until another chart could keep it company in the post. Galle took charge of the telescope and described the configurations and magnitudes of the stars in the field of view, with d'Arrest checking Galle's observations on the chart. Soon Galle described the position of an eighth-magnitude star; d'Arrest immediately exclaimed: "That star is not on the chart." Subsequent observations confirmed its planetary character; the hypothetical planet had become a reality.

Naturally, there was great enthusiasm in France; Arago (director of Paris Observatory), referring to Le Verrier's achievement, declared that the discovery of the new planet "would remain one of the most magnificent discoveries of astronomical theory, one of the glories of the French Academy and one of the noblest titles of his country to the

gratitude and admiration of posterity."

Into this atmosphere of rejoicing came immediately the first rude shock in the form of a letter from Sir John Herschel to the Athenoeum, making the first public reference to Adams and to his investigations; Herschel's knowledge of these was limited to the information briefly given by Airy at the meeting of the Board of Visitors three months previously. A second shock was provided by Challis' announcement that he had been engaged at Cambridge in the search for the hypothetical planet since the end of July, and that since its optical discovery at Berlin a scrutiny of his observations had revealed the fact that he had actually observed it on four occasions. Except for a comparison of his observations on July 30 and August 12, Challis made no attempt to discuss the fruits of his toil, despite the sense of urgency, which Airy's importunity and his own knowledge of Le Verrier's June paper would have made imperative to one who had even a modicum of faith in the results of mathematical analysis. Comparing his observations on August 12 with those on July 30, Challis noted that the first thirty-nine stars on the former date agreed with the observations on July 30; if he had gone on to star number forty-nine he would have seen that this star was absent from the records of July 30; this star was the planet. On September 29, before the news of the discovery at Berlin reached Cambridge, Challis, impressed with Le Verrier's insistence that the planet would show an unmistakable disk, noted against a star: "It seems to have a disc"; this again was the planet. The fourth observation had been made on August 4.

As might have been expected, consternation reigned in Paris at what appeared to be an impudent claim to priority of discovery made on behalf of Adams. No wonder that Arago announced pontifically that Adams had "no right to figure in the history of the new planet, neither by a detailed citation, nor even by the slightest allusion." The defense of Le Verrier was promptly undertaken by Airy who, in a letter to the former, declared: "You are to be recognized beyond doubt as the real predictor of the planet's place." A little later he wrote: "No one will dispute the completeness of your investigations and the fairness of your moral convictions as to the accuracy and certainty of the results. With these things, the produce not only of a mathematical but also of a philosophical mind we have nothing which we can put in competition. My acknowledgment of this will never be wanting." It is to be remembered that Airy's knowledge of Le Verrier's work was confined to the three abstracts printed in *Comptes Rendus,* for the full mathematical investigation was published only towards the end of 1846. Later, Airy's opinion was less dogmatic. Writing to Biot in June, 1847, he says: "I assure you that I have a very high opinion of Mr. Adams and that upon the whole I think his mathematical investigations superior to M. Le Verrier's. However, both are so admirable that it is difficult to say."

In the weeks following the discovery of Neptune, the French press was exceedingly bitter in its attacks on Airy, Challis, and Herschel. English men of science were dumbfounded at the revelations of Airy's and Challis' shares in the transaction. Considering the latter first, we have his own word that he had very little faith in the outcome of theoretical investigations for detecting a new planet; he seemed to undertake the laborious series of observations merely because Airy was firm on the matter; and when he had embarked on the observational program it never occurred to him to discuss his observations as they proceeded—except for the instance recorded earlier, and the comparison in this case was merely a test of the adequacy of the two separate observational methods he had adopted. Challis comes out of the Neptune episode as a skeptic and procrastinator, perhaps not earning, however, the almost brutal judgment passed on him by the historian of the Royal Astronomical Society.

It was Airy, however, on whom the greatest weight of criticism fell. His long silence as to Adams' investigations, his alleged 'snubbing' of Adams, but above all his fulsome praise of Le Verrier without any accompanying reference to Adams were the main points of accusation. Le Verrier, indeed, deserved every eulogy from whatever quarter it came; but the apparently pointed neglect of a young Cambridge graduate by the acknowledged head of British astronomy was something that no fair-minded person could understand. After reading the private papers of Adams and the contemporary literature, I am convinced that criticism of Airy was on some points unfair and unjustifiable; but I am equally convinced that his treatment of Adams in general was unbecoming to the leading astronomer of his generation. In any event some kind of action was called for. At the famous meeting of the Royal Astronomical Society on November 13, 1846, Airy read his "Account of some Circumstances Historically connected with the Discovery of the Planet exterior to Uranus"; he was followed by Challis, who described his observations at Cambridge, and finally by Adams, who outlined his theoretical investigations. In his "Account," Airy claimed to know the history of the whole business; but it is significant that of Adams he scarcely knew anything. In asking Adams for permission to insert in his "Account" such corespondence as had passed between them (this was Airy's second letter to Adams, the first being that containing the radius vector query) he addressed him as "The Rev. W. J. Adams"! Moreover, until then Airy, on his own confession, had met Adams only twice; on the first occasion he had forgotten where; on the second, in company with Hansen, on St. John's Bridge on July 2, 1846; each interview lasted no more than a couple of minutes. It seems extraordinary that on the second occasion—Le Verrier's second paper was by then known to both—two of the world's most eminent astronomers should meet the young Johnian without making some reference to his

share in disentangling the most baffling problem in contemporary astronomy. Airy's "Account" contained several extraordinary passages, full of the liveliest eulogies of Le Verrier, but almost destitute of the deserved recognition of Adams' achievements. At the conclusion of his "Account," which in some measure must be reckoned a defense of his own conduct, Airy made one remarkable statement (its significance seems to have been overlooked by all previous commentators) to the effect that if Adams and Le Verrier had not adopted Bode's rule of distances they would never have arrived at the elements of the orbit. It is legitimate to ask if Airy really understood the problem of inverse perturbations so confidently and successfully tackled by Adams and Le Verrier, for unless some value of the semi-major axis, a, of the unknown planet is assumed, the problem becomes intractable owing to the complicated way a enters into the expression of the disturbing function. It was obvious to Adams and Le Verrier who, it must be remembered, were supremely confident of the existence of an exterior planet, that a 'trial and error method' was the only one to be adopted. They both soon found that the value of a must be considerably reduced—in other words that Neptune provided an exception to Bode's rule. If the rule had never been heard of, they must of necessity have adopted some value for a and proceeded on the lines of their respective investigations.

Challis had a most unenviable task at the meeting. A few days before, he had written to Airy: "I am in difficulties about this report (for the meeting) and should be glad to see some means of getting out of it." His 'report' was a confession of skepticism and procrastination. Adams' share in the proceedings took the form of a masterly account of his own investigations, concluding with a generous tribute to Le Verrier. It should be stated that he never took any part in the controversy that raged so long around his name, nor did he ever utter a harsh word about those to whom an inexperienced youth might have expected to look for guidance, advice, and encouragement.

Perhaps the greatest slight to which Adams was subjected was the award of the Copley Medal of the Royal Society to Le Verrier on November 30, 1846. In this award the discovery was attributed to Le Verrier alone without any refernce to Adams, despite the fact that those responsible for the award must have known about the proceedings at the Royal Astronomical Society meeting more than a fortnight before. The Royal Society was evidently of Arago's opinion that Adams had no right to figure in the history of the discovery of Neptune in any way. The Society, however, made amends by awarding to Adams the Copley Medal in 1848. The Royal Astronomical Society was saved by its by-laws from perpetrating a similar injustice. One medal and only one could be awarded; it was proposed, however, to waive the by-law protem, with the obvious intention of honoring both Le Verrier and Adams. This proposal in council was defeated. A resolution to award

the Medal to Le Verrier alone was carried by 10 votes to 5, but as the by-laws stipulated a 3 to 1 majority the proposal was inoperative. Thus, there was no award by the leading astronomical society in the world for the most spectacular discovery in the history of astronomy.

Honors were immediately—and deservedly—showered on Le Verrier from all quarters. Recognition of Adams' achievements was much more tardy. It is worthy of mention that on the occasion of Queen Victoria's visit to Cambridge in the summer of 1847 the Vice-Chancellor was informed that "Her Majesty had commanded the honor of knighthood to be offered to Mr. Adams"; but Adams, against the advice of Prof. Adam Sedgewick, whom he consulted, modestly prayed to be allowed to decline the honor. About the same time he also declined the chair of natural philosophy at St. Andrews.

A subsidiary controversy—intimately connected, however, with the French claim on behalf of Le Verrier for the undivided credit of discovery—raged around the name to be given to the planet. It is usually stated that the name of Neptune (with a trident as the astronomical sign) was at first mutually agreed upon by Le Verrier and the Bureau of Longitudes. M. Danjon, director of the Paris Observatory, has recently informed me that there is no record in the Bureau confirming this; the name was certainly suggested by Le Verrier himself a few days after the discovery of the planet. But a little later, Le Verrier persuaded Arago to accept the discoverer's privilege of naming the new planet. Arago immediately announced that he had decided to name the new planet "Le Verrier," adding that in consequence there must be a wholesale renaming of the planets hitherto discovered (Uranus and five minor planets) in accordance with this new principle of attaching the discoverer's name to the planet discovered by him; for example, the name "Uranus" must now be changed to "Herschel." Both he and Le Verrier vowed that the new planet would never be referred to by them except by the name of "Le Verrier." At this time Le Verrier's complete mathematical investigations were presented to the Academy with the title "Researches on the Motion of the Planet Herschel (formerly Uranus)"; in the body of this large memoir the planet is referred to as Uranus, and Le Verrier explained in the preface that owing to the advanced state of printing it was impossible to effect the change throughout. This clumsy device of associating the new planet with Le Verrier alone met, quite naturally, with the unanimous disapproval of astronomers in other countries; it is perhaps worthy of mention that the Royal Astronomical Society Club—a festive body not usually prone to the discussion of serious subjects—was quite prepared to accept any mythological name proposed by Le Verrier. Soon the French astronomers were constrained to fall into line, and the name of Neptune passed eventually into established nomenclature.

The question as to whether Adams or Le Verrier should be accorded

priority of discovery agitated scientific circles for several months. There could be no doubt as to the relevant events and their sequence. But could Adams' communications to Airy and Challis be regarded as 'publications,' for no one disputed the fact that Le Verrier was the first to get into print? The fact that Adams was engaged in investigations of a trans-Uranian planet was known to various reputable astronomers in Britain and was the subject of general comment in Cambridge; it is true, of course, that Airy and Challis were alone familiar, at some time or other, with the main features of Adams' investigation and in possession of such information as to lead to the detection of Neptune in October, 1845. The doctrine of priority was stated unequivocally by Biot in terms of "the common and imprescriptible law without which no scientific title could be assured that a discovery belongs to him who proclaims and publishes it to all." It is to Airy's credit that he explicitly denied the existence of such a law. Scientific workers in the past had adopted various expedients to ensure their titles to a discovery —the anagrams of Galileo and Huygens relating to the peculiar appearance of Saturn and to the rings of the planet are well-known instances. In later times the device of the 'sealed packet' became almost universal; Faraday, Wheatstone, and Brewster adopted this expedient, which was even more popular with the Paris Academy of Sciences for, in 1846, no fewer than ninety were deposited and recorded in *Comptes Rendus*. There was thus some reason for the claim on behalf of Adams, for was not Airy the custodian of the young astronomer's results, and was he not responsible (in the last resort) for jogging the apathetic Challis to activity? The impartial verdict of the illustrious Struve may be quoted: "It cannot be denied that Mr. Adams has been the first theoretical discoverer of Neptune, though not so fortunate as to effect a direct result of his indications." In this centenary year there is no need for us to try to settle this vexed—and interesting—question of priority; rather do we hail Adams and Le Verrier as the co-equal sharers of one of the greatest triumphs of science.

But the element of dramatic surprise had not yet been exhausted. Using Challis' observations at Cambridge, Adams proceeded to calculate the elements of the new planet's orbit, as accurately as such observations permitted; the results proved to be in excellent agreement with results derived afterwards from more abundant observational material. It is interesting to note that Airy had little faith in such attempts. Writing to Adams near the beginning of 1847, he says: "I cannot conceive that you can obtain from the observations made at the now expiring appearance of the new planet any determination of its actual distance from the sun sufficiently accurate to be of the smallest service to you." In this opinion he showed a sad lack of appreciation of the possibilities of determining the orbital elements and, what was of the greatest importance, of using these for the search of 'ancient' observa-

tions, as was so successfully done in the case of Uranus. The surprising result of Adams' and other calculations was the comparatively small value of the semi-major axis of Neptune's orbit: this was thirty astronomical units as against about thirty-five used in Adams' and Le Verrier's solutions; no wonder that Peirce of Harvard was led to declare that Neptune was not the planet resulting from mathematical analysis and that its discovery must be accounted a happy accident. The arguments in refutation of such a suggestion are somewhat technical, and we must be satisfied on the present occasion with the mere statement that Neptune is indeed the fruit of Adams' and Le Verrier's genius.

Adams, now armed with satisfactory orbital elements, himself examined old catalogs in an attempt to discover 'ancient' observations of the planet, but his efforts were unsuccessful. However, Walker at Harvard and Peterson at Altona discovered an old observation of Neptune made by Lalande on May 10, 1795; this observation was marked 'doubtful,' and it seemed bad luck that, out of so many thousand observations of stars, the only one that mattered in this connection should be reckoned by Lalande to be unworthy of confidence. Subsequent reference to Lalande's manuscripts revealed the interesting fact that Lalande had observed the 'star'—as he believed it to be—on May 8, as well as on May 10; as the observations on the two nights were discordant he discarded the first and included only the second in his catalog, labelling this as 'doubtful.' Instead of one unsatisfactory position of the planet, astronomers were now provided with two satisfactory positions, and these contributed very substantially to the accurate determination of the planet's orbit.

The subsequent careers of Le Verrier and Adams may be briefly indicated. For the former, a professorship of celestial mechanics was especially created in Paris; later he became director of the Paris Observatory. Le Verrier received the Gold Medal of the Royal Astronomical Society on two occasions in recognition of his masterly investigations in planetary theory.

Adams occupied the chair of mathematics at St. Andrews for a year, returning to Cambridge in 1859 as Lowndean professor; in 1861 he succeeded Challis as director of the Cambridge Observatory, where he resided until his death in 1892. He was president of the Royal Astronomical Society during 1851-53 (perhaps the most youthful occupant of the chair in the history of the Society) and again during 1874-76. In 1881 he was offered by Gladstone, then Prime Minister, the post of Astronomer Royal in succession to Airy, but this he declined. Adams' contributions to celestial mechanics were outstanding—perhaps no one has ever possessed such a thorough grasp of this most intricate subject, in which he was the acknowledged master.

G. B. Airy and the Discovery of Neptune*

By SIR H. SPENCER JONES

The account by Prof. W. M. Smart of the discovery of Neptune, of which a summary was published in *Nature* for November 9, depicts the part played by G. B. Airy, the Astronomer Royal, in a most unfavorable light. He describes the treatment of Adams by Airy as "unbecoming to the leading astronomer of his generation." Prof. Smart's verdict is not, in my opinion, justified, and I feel that, for the sake of historical accuracy, a reply is needed.

In judging Airy's actions, it is necessary to remember the tremendous load of work which he carried. Besides attending to all the details of the work of the Royal Observatory, he maintained an extensive correspondence with astronomers in all parts of the world and was consulted on a great variety of general scientific questions outside the range of his strict official duties. No man could have been more meticulous in replying promptly to all letters and inquiries. An examination of Airy's day-book shows that in the period covered by the investigations of Adams, Airy visited France for the purpose of examining and reporting upon the design and construction of the breakwater at Cherbourg; he went to York to see experiments on the running of engines; he visited Portsmouth to inquire into and report upon the defects of the engines of H.M.S. *Janus;* he was occupied with the Tidal Harbor Commission and he was frequently called to London for meetings of the Railway Gauge Commission, the draft report of which he prepared.

Airy first learnt that Adams was working on the theory of Uranus from Prof. Challis, who wrote to Airy in February, 1844, asking for the errors of longitude of Uranus, as indicated by the Greenwich observations, for the years 1818-26. Airy by return of post sent the Greenwich data not merely for those years, but also for the whole period 1754-1830, data invaluable for the purpose of the investigation.

Adams twice called at Greenwich in the course of his investigations in the hope of seeing the Astronomer Royal and discussing the results he had obtained. It would have been a matter of ordinary courtesy for a young man like Adams, personally unknown to the Astronomer Royal, to have written and asked for an appointment, but Adams on each occasion called without any previous notice. On the first occasion, towards the end of September, 1845, Adams called at Greenwich and left a letter of introduction from Challis; Airy was then in France on the Cherbourg breakwater investigation. Immediately on his return, he wrote to Challis and said: "Would you mention to Mr. Adams that I am very much interested with the subject of his investigations, and that I shall be delighted to hear of them by letter from him." This letter

*Published in *Nature*, December 7, 1946.

should surely have encouraged Adams to write. On the second occasion, on October 21, 1845, Airy was in London attending a meeting of the Railway Gauge Commission. Adams left his card and said that he would call later. The card was taken to Mrs. Airy, but the message was not given to her. When Adams made his second call, he was informed that the Astronomer Royal was at dinner; there was no message for him and he went away feeling mortified. This visit is not mentioned in Airy's day-book, and it is clear from Airy's private correspondence that he was not told either of Adams' intention to call again or of his second call. It may also be mentioned that this visit of Adams was made a few days before Mrs. Airy gave birth to a son, Osmund.

Adams left at the Observatory a paper with a summary of his results, and a comparison between the observed longitudes of Uranus and those computed from his theory. On November 5, Airy wrote to Adams putting his famous query about the errors of the radius vector of Uranus. Adams never replied to this letter. In a later letter to Airy of November 18, 1846, he stated how deeply he regretted his neglect and mentioned that he had always experienced a strange difficulty in writing letters. But to a man so methodical and precise as Airy, it was a barrier to any further communication. As Airy afterwards wrote to Challis, "It was clearly impossible for me to write to him again." This is why Adams' statement remained, in Prof. Smart's words, "in Airy's pocket for eight months."

Prof. Smart seems to regard Airy's query as trivial. Airy was, of course, thinking of the possibility that perturbation by an unknown planet might not be the only possible cause of the irregularities in the motion of Uranus. His views were clearly expressed in a letter to Challis (December 21, 1846). "There were two things to be explained, which might have existed each independently of the other, and of which one could be ascertained independently of the other: *viz.*, error of longitude and error of radius vector. And there is no *a priori* reason for thinking that a hypothesis which will explain the error of longitude will also explain the error of radius vector. If, after Adams had satisfactorily explained the error of longitude he had (with the numerical values of the elements of the two planets so found) converted his formula for perturbation of radius vector into numbers, and if these numbers had been discordant with the observed numbers of discordances of radius vector, then the theory would have been false, not from any error of Adams' but from a failure in the law of gravitation. On this question therefore turned the continuance or fall of the law of gravitation."

Prof. Smart mentions that even so late as 1844 Airy regarded as possible that gravitation might not be exactly according to the inverse square of the distance. It is perhaps well to recall that, fifty years later, the same suggestion was seriously examined by Simon Newcomb and

otner eminent astronomers in the endeavor to explain the anomaly in the motion of the perihelion of Mercury.

It is also stated that towards the end of June, 1846, Le Verrier applied to Airy for assistance in the search for the planet, and that this request for practical aid passed unheeded. What were the facts? Airy had written to Le Verrier putting to him the query about the errors of radius vector which he had previously put to Adams, and had at once received a satisfactory reply. The assurance that the hypothesis of an unknown planet accounted for the errors of both longitude and radius vector of Uranus had convinced him of the reality of the planet's existence. He considered that the telescopes at Greenwich were probably of insufficient size to detect the planet and that the Northumberland telescope at the Cambridge Observatory was the most suitable for the purpose of the search. He therefore wrote to Challis on July 9, 1846, inquiring whether he could undertake the search and, if not, whether he would superintend the examination if Airy supplied him with an assistant from Greenwich for the purpose. He concluded by saying, "The time for the said examination is approaching near."

When Challis informed Airy that he would undertake the search, Airy drew up as a guidance for Challis his "suggestions for the examination of a portion of the Heavens in search of the external planet which is presumed to exist and to produce disturbances in the motion of Uranus" (dated July 12, 1846). In sending this paper to Challis he wrote, "I only add at present that, in my opinion, the importance of this inquiry exceeds that of any current work, which is of such a nature as not to be totally lost by delay." Airy could not have done more to further the search and to impress upon Challis its urgency. There is little doubt that if the search had been carried out by an assistant from Greenwich, the planet would have been found, for it was an essential part of Airy's system that reduction of observations proceeded *pari passu* with the observations themselves.

As regards the actual researches of Adams and Le Verrier, full abstracts of Le Verrier's investigations had been published in the *Comptes Rendus,* but neither Airy nor Challis had received anything from Adams beyond the bare summary of his results; they knew nothing of the methods he had employed.

After the discovery of the planet by Galle at Berlin, Airy wrote to Le Verrier and informed him that collateral researches, which had led to the same results as his own, had been made in England, and that they had been known to him earlier than those of Le Verrier. His "Account of some circumstances historically connected with the discovery of the planet exterior to Uranus" presented to the Royal Astronomical Society on November 13, 1846, left no doubt about the priority of the researches of Adams. In a letter of later date to Biot, Airy wrote, "I

believe I have done more than any other person to place Adams in his proper position."

Prof. Smart agrees that the contemporary criticism of Airy, made in ignorance of many of the facts, was on some points unfair and unjustifiable. In my opinion, his verdict that Airy's treatment of Adams was unbecoming is equally unjustifiable.

Reply by W. M. Smart

The Astronomer Royal does not see eye to eye with me in my judgment of Airy, in connection with the Neptune controversy, as expressed in my article in *Nature* for November 9. This article, which was written in response to an editorial request, was a summary of the two addresses —dealing with different aspects of the discovery of Neptune—which I gave at the centenary commemoration on October 8; these addresses were themselves a summary of a fairly long "essay" (if I may call it so) written at the invitation of the Council of the Royal Astronomical Society and accepted, as I understand, by the Council for eventual distribution to the fellows in one of the Society's publications. The "essay" is a historical study of events of a century ago, and I was very conscious throughout its preparation that I must follow the methods of the historian as efficiently as I knew how. The job of the historian, as I see it, is to elicit facts, to present these in proper form, and to paint as accurate and complete a picture as possible. The "essay" was accordingly built up on a very large amount of historical documents—I explain in the "essay" how many of these became available, for the first time, for a study of the Neptune controversy, in which Sir Harold's great predecessor was in many ways the dominant figure.

All this, it seems to me, must be said before one turns to the criticism of the Astronomer Royal. Sir Harold's arguments, when documentary evidence is invoked, are based on Airy's letters alone. Most of his quotations will also be found in my "essay," if—in one or two instances—not as direct quotations then as transcriptions of them. There is no suggestion in my article or "essay" that Airy was to blame for Adams' failure to see the former on the occasion of his abortive visit to the Royal Observatory in October, 1845—it was far otherwise—and as to the famous query about the "radius vector," Adams never failed to reproach himself for not replying to Airy, although he was convinced that the matter was "trivial," an opinion shared at the time by Challis.

The main questions are: Why did Airy claim to know the whole history of the business? Why did he declare unambiguously that Le Verrier must be regarded as the real "predicter" of the planet? Why did he affirm that there was no one (in England) in competition, as regards scientific insight, with Le Verrier, etc.?

It is to be remarked that Airy's correspondence with Le Verrier was understood by him to be "private," and he was exceedingly indignant—

and justly so—when his letters were published in the French press without his sanction being even asked. Later, Airy described Adams as his "oracle" in all matters relating to lunar and planetary theory; but this has nothing to do with the Neptune controversy as a historical episode. Airy was unjustly criticized on many points, as the Sedgwick correspondence makes abundantly clear, and as I hope my article and "essay" demonstrate.

Any judgment on Airy's actions must be based, not on his letters alone, but on the whole corpus of contemporary documents. I do not claim that my "essay" is the last word on the subject, but I do claim that, whatever its faults may be, it was written as a purely historical study with all the implications that this description suggests.

The Planets in July and August, 1947

By RAYMOND H. WILSON, JR.

NOTE: The time employed is Central Standard Time unless otherwise indicated. The phenomena have been chosen and described for the North American continent, and especially for the United States. The basic data have been taken principally from the *American Ephemeris and Nautical Almanac*.

Sun. Although the season of highest temperature occurs in the middle of this period, the sun will be steadily returning southward, being only 9 degrees north of the equator at the end of August.

Moon. The phases of the moon will occur as follows:

Full Moon	July	3	5 A.M.
Last Quarter		11	5 A.M.
New Moon		17	10 P.M.
First Quarter		24	5 P.M.
Full Moon	August	1	8 P.M.
Last Quarter		9	2 P.M.
New Moon		16	5 A.M.
First Quarter		23	7 A.M.
Full Moon		31	11 A.M.

No bright stars will be occulted.

Evening and Morning Stars. Venus and Mars will be the most conspicuous morning stars, although Venus comes too close to the sun in August. Jupiter, near quadrature, will be the most brilliant evening star. Mercury will be a visible morning star early in August, while Saturn will be very near the sun, an evening star in July and a morning star in August.

Mercury. Rapid retrograde motion will lead Mercury from the evening sky to inferior conjunction; then through the morning sky to greatest elongation, nearly 20 degrees west of the sun, on August 3. With clear sky it should be visible at this date in the morning twilight just above the sunrise point and about 10 degrees south of the star Pollux. Then, moving eastward again, it will overtake the sun in superior conjunction at the end of the period.

Venus. Venus will be steadily overtaking the sun, from a position rising an hour before it at the beginning of this period, to invisible nearness at the end.

Mars. Mars is still keeping an eastward speed almost equal to that of the sun, although the sun has gained a lead such that Mars now rises about 3 hours before it. In the middle of July the red planet may be confused with Aldebaran, when it passes less than 5 degrees north of that star and is only slightly fainter in magnitude. In August Mars will pass some 15 degrees north of the red star Betelgeuse.

Jupiter. This most conspicuous object of the southern evening sky will remain almost stationary in Libra, beginning its eastward motion again on July 16. It passes quadrature on August 12, when it will be just west of the meridian at sundown. There will be very close conjunctions with the moon on July 25, when the planet will be south of the moon, and on August 22, when it will be north.

Saturn. Since it passes conjunction with the sun on August 5, Saturn will be practically invisible during most of this period.

Uranus. Uranus will be moving slowly eastward, about 2 degrees north of ζ Tauri. On July 2 it will be only half a degree north of Venus; and on the morning of August 6, only a few *minutes* southwest of Mars, then appearing as a satellite of the red planet.

Neptune. Neptune will be moving slowly eastward, about 3 degrees southwest of γ Virginis.

Department of Mathematics, Temple University, Philadelphia, Pa.
May 10, 1947.

Asteroid Notes

By HUGH S. RICE

The asteroid situation is gradually but steadily improving. Many of the opposition ephemerides are being computed, and the computation is being distributed among a number of institutions in the U. S. and Europe. We have heard from Dr. Gondolatsch of the German headquarters, which is now at Heidelberg. During the winter they were occupied by caculations for the *Astronomische Jahrbuch,* and in February had begun work on the minor planets.

We have at hand ephemerides of many asteroids for the first half of this year; these are distributed by Dr. Cunningham of the Students' Observatory at Berkeley, California. We have not yet received any data for the third quarter of 1947. Hence we can offer at this time only the places of two planets, as supplied by Dr. Brouwer of the Yale University Observatory.

3 JUNO comes to opposition on June 17. The planet is then in eastern Ophiuchus, and the visual magnitude is about 10. 216 KLEOPATRA comes to opposition on July 27. The planet is found at the Aquila-Delphinus boundary in July; and near the end of July and in August the visual magnitude is about 10. Kleopatra was discovered on April 10, 1880, by Palisa at Pola, Italy. It was then of magnitude 11, and was observed one month. Palisa is one of those who have discovered numerous asteroids, and the co-author of the Wolf-Palisa charts, which show stars to a very faint magnitude. For a period of 10 years Kleopatra was not observed; then it was rediscovered in 1905, and again in 1910. Its average magnitude at opposition is 10. The asteroid is interesting in having a high inclination

to the ecliptic plane of 13°, and a high eccentricity of 0.251; the semi-major axis has a length of 2.79 astronomical units.

ASTEROID EPHEMERIDES

For 0h U.T. Equinox of 1947.0

1947		3 JUNO α (h m)	3 JUNO δ (° ′)	216 KLEOPATRA α (h m)	216 KLEOPATRA δ (° ′)
June	14	17 41.4	−4 36	20 43.7	+0 22
	19	17 37.1	−4 34	20 42.9	+0 52
	24	17 32.7	−4 34	20 41.5	+1 18
	29	17 28.5	−4 38	20 39.5	+1 41
July	4	17 24.5	−4 45	20 37.0	+1 59
July	9	17 20.8	−4 56	20 33.9	+2 13
	14	17 17.4	−5 9	20 30.5	+2 22
	19	17 14.4	−5 25	20 26.6	+2 25
	24	17 12.0	−5 43	20 22.5	+2 22
	29	17 10.0	−6 4	20 18.2	+2 15
Aug.	3	17 8.6	−6 26	20 13.9	+2 1
	8	17 7.7	−6 50	20 9.7	+1 43
	13	17 7.4	−7 14	20 5.7	+1 20
	18	17 7.6	−7 40	20 2.0	+0 53
	23			19 58.8	+0 23

Hayden Planetarium, American Museum of Natural History, New York, N. Y., May 22, 1947.

Occultation Predictions for July and August, 1947

(Taken from the *American Ephemeris*)

Date 1947	Star	Mag.	IMMERSION Greenwich C.T. (h m)	IMMERSION a (m)	IMMERSION b (m)	IMMERSION Angle E from N (°)	EMERSION Greenwich C.T. (h m)	EMERSION a (m)	EMERSION b (m)	EMERSION Angle E from N (°)
OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′										
July 11	33 Ceti	6.2	6 32.7	−1.1	+1.0	110	7 14.5	−0.1	+2.8	183
29	136 G.Ophi	6.3	1 53.5	−2.1	−0.3	107	3 23.7	−1.9	−0.8	273
30	66 B.Sgtr	4.7	0 41.4	−1.3	+0.2	124	2 1.4	−2.3	+0.6	253
Aug. 5	336 B.Aqar	6.5	8 35.4	−2.5	−0.9	95	9 34.0	−0.2	+1.8	191
11	53 Taur	5.4	5 2.1	+0.3	+1.2	86	5 51.5	+0.3	+1.7	233
OCCULTATIONS VISIBLE IN LONGITUDE +91° 0′, LATITUDE +40° 0′										
July 11	33 Ceti	6.2	6 19.1	−0.2	+1.1	102	7 4.8	−0.1	+2.4	201
29	136 G.Ophi	6.3	1 21.0	−1.3	−0.2	129	2 43.5	−2.4	+0.3	263
29	151 G.Ophi	6.0	5 6.9	−2.7	−3.8	162	5 39.3	−0.5	+1.7	206
30	66 B.Sgtr	4.7	0 30.6	+0.4	−1.3	160	1 13.1	−2.7	+2.9	225
30	69 G.Sgtr	6.3	7 0.6	−0.6	+0.3	39	7 56.9	−1.6	−2.4	305
Aug. 5	336 B.Aqar	6.5	7 54.0	−1.9	+1.1	65	9 16.7	−1.3	+1.2	220
20	k Virg	5.9	1 24.4	−0.2	−3.0	180	1 57.0	−0.7	−0.6	240
29	56 B.Capr	6.3	0 14.9	−1.9	+3.5	24	0 55.4	−0.6	−0.5	321
OCCULTATIONS VISIBLE IN LONGITUDE +120° 0′, LATITUDE +36° 0′										
July 1	20 Ophi	5.8	8 46.4	..	..	168	9 15.5	−0.5	+2.3	208
11	*f* Psc	5.3	10 14.1	−0.9	+1.8	64	11 25.1	−1.0	+2.0	226
Aug. 5	336 B.Aqar	6.5	7 4.1	−1.0	+2.3	39	8 19.4	−1.9	+1.3	259
11	247 B.Taur	5.7	8 25.8	+0.1	+0.6	110	9 4.8	+0.6	+2.0	209
23	11 H.Libr	5.5	3 29.4	−2.0	−1.0	102	4 51.7	−1.4	−1.8	299
24	ρ Ophi *m*	4.8	5 0.6	−1.8	−1.8	122	6 15.3	−0.9	−1.0	259

Date 1947	Star	Mag.	IMMERSION Greenwich C.T.	*a*	*b*	Angle E from N	EMERSION Greenwich C.T.	*a*	*b*	Angle E from N
			h m	m	m	°	h m	m	m	°
Occultations Visible in Longitude +98° 0′, Latitude +30° 0′*										
July 3	ϕ Sgtr	3.3	8 43.0	—2.7	—1.8	117	9 51.0	—0.5	+0.6	223
6	33 Capr	5.5	11 10.2	—0.9	+1.0	38	12 23.3	—1.3	—0.7	261
11	*f* Pisc	5.3	10 51.4	..	..	122	11 17.1	..	..	159
29	136 G.Ophi	6.3	1 27.5	+0.6	—3.3	174	2 9.5	—4.4	+3.3	228
30	68 G.Sgtr *m*	6.2	7 12.6	..	..	357	7 24.1	..	..	344
Aug. 5	336 B.Aqar	6.5	7 27.1	—2.3	+1.3	72	8 51.2	—1.4	+2.0	212
20	46 Virg	6.1	1 27.4	—1.4	—0.7	78	2 15.9	—0.4	—2.7	343
24	BD—22°4159 *m*	7.4	3 49.6	—1.7	—1.2	97	0 48.0	—0.9	+0.6	287

*Computed by Edgar W. Woolard and Paul Herget; communicated by Commodore J. F. Hellweg, Superintendent U. S. Naval Observatory.

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

It has been some time since the more routine work of our members has received much attention in these Notes, as the great shower of last October overshadowed other matters. While there are still more data on that, and all the discussion still to be published, it seems wise for a change to call attention to what a few of our active members are now doing. Hence reports from three, on work done in 1947, and from two on work done in 1946 but recently received, will be tabulated and briefly discussed.

Taking the 1946 work first, H. A. Burns observed the Orionids on three dates. His maps give an excellent radiant at R.A. 88°, Decl. +18½° (A.M.S. No. 1957) for Oct. 20.7, based upon 11 meteors. On Oct. 22.7 there is another apparently good radiant (A.M.S. No. 1958), based also upon 11 meteors, at R.A. 84°, Decl. +12°, and hence quite far from the usual position, if it refers to the Orionids as is certainly probable. It is true that 7 of these meteors would also fit the expected position for the data well, and 2 more fairly well, but the only two whose paths are critical for determining the right ascension project several degrees west of the expected position. If these two are not Orionids, the radiant

is in any case indeterminate from the observations in question. Two minor radiants, based on 3 meteors each, are found at R.A. 54°, Decl. +14°, and R.A. 81°, Decl. +20°. These are not given A.M.S. numbers because they are based upon so few meteors, but they may be confirmatory of others. On Oct. 23, 3 of the 4 plotted meteors are Orionids, but their paths happen to be too nearly parallel for the derivation of a radiant.

In tabulating the work of Prof. Khan, it should be remembered that actually he observed on many more nights than appear in the table, but most of these were for periods of 30 minutes or less. His total for the year was 800 meteors. He sends in the record sheet with all the coordinates of every meteor but not the maps. This means that, to derive his radiants, all his meteors would have to be replotted from their coordinates—a rather colossal task for which I do not have time myself at present and no assistant to do it for me. I am hence now unable to give the radiants which would undoubtedly come from his excellent work, but hope to do so in future. He is our most active member and sets a fine example to the rest of us, as well as to his fellow countrymen who are interested in science.

In this country, one of our new members is making an excellent record—namely Tommy Scott of Nauvoo, Alabama. Since the beginning of 1947 he has observed on 14 nights over periods of from 2 to about 4 hours, besides nights when casual observations were made or clouds, etc., stopped him. From his work, I have derived the following probable radiants:

A.M.S. No.	Date		R.A.	Decl.	Meteors	Accuracy
1959	1947 Jan.	26.6	114°	+68°	4-5	fair
1960	Feb.	14.6	141	+25	4	fair
1961	Mar.	21.6	155	— 6	3	good
1962		21.6	170	—18	3	good
1963		21.6	190	+ 5	5	fair
1964	Apr.	14.6	202	+29	4	fair
1965		17.6	197	+59	6	fair
1966		17.6	239	—14	5	good

Three others, without numbers due to too few meteors, etc., are:

Jan.	26.6	106°	+23°	3	good
	27.6	141	+42	4	poor
Mar.	10.6	179	+22	2	good

His total for 1947, all plotted, is 250 to May 9.

Wm. J. Siekman, Jr., sent in a good record for April 20, but he too worked out the coordinates of all meteors and did not send in his maps. Hence no radiant can be derived until the latter are received or somebody takes time to replot. Vincent Anyzeski's work yields two radiants as follows:

A.M.S. No.	Date		R.A.	Decl.	Meteors	Accuracy
1967	Mar.	16.7	169°	+ 4°	4	good
1968	Apr.	22.6	270	+36	8	fair

Plans that he made for cooperative work on the Lyrids with a group at Smith College under Miss Helen Pillans were much spoiled by clouds, as the South Hadley group could only plot 6 meteors on April 19/20.

As the Moon was full on May 5, it pretty well spoiled any plans for observing the Eta Aquarids. The same conditions will apply to the Delta Aquarids at the end of July, but the Perseids maximum in August will fall after last quarter and

so the moonlight conditions should be favorable. This, with summer temperatures, should induce even the most indifferent of our members to some activity in the period August 9 to 13, inclusive. Those who need maps are asked to apply some weeks in advance and not wait until the date has almost arrived. If the A.M.S. is to prosper, more of our members need to do some regular observing. One cannot go out once per year and plot meteors decently; practice is most necessary.

Observer and Station	Date	Began	Ended	Min.	Met.	F	Rate Unc.
	1946						
Burns, H. A.	Oct. 20	12:10	14:10	120	20	1.0	10.0
Spring Valley, N. Y.	22	12:00	14:15	135	32	1.0	14.2
	23	12:26	13:48	82	5	0.9	3.6
	1947						
Siekman, Wm. J., Jr.	Jan. 4	16:30	17:30	60	6	1.0	6.0
Batavia, Ill.	Apr. 20	13:00	15:30	150	22	1.0	8.8
Anyzeski, V.	Feb. 27	8:45	9:50	65	6	1.0	5.6
New Haven, Conn.	28	8:15	10:15	120	9	0.7	4.5
	Mar. 12	8:00	10:30	120	9	0.9	4.5
	16	10:15	11:30	75	10	0.6	8.0
	17	11:30	12:00	30	4	0.9	8.0
	28	10:	11:	60	3	0.4	3.0
	Apr. 22	9:30	12:00	150	10	0.8	4.0
Scott, T.	Jan. 22	7:40	9:50	130	10	1.0	4.6
Nauvoo, Ala. CST	26	7:30	10:15	165	9	0.9	3.3
	27	7:45	10:15	150	15	0.8	6.0
	Feb. 14	8:00	10:45	165	9	0.8	3.3
	Mar. 10	8:15	10:30	135	12	0.9	5.3
	14	8:05	10:45	160	11	0.7	4.1
	19	8:00	8:40	40	7	...	10.5
	21	8:15	11:15	180	21	1.0	7.0
	25	8:10	11:00	170	21	0.9	11.5
	Apr. 14	8:15	12:15	215	22	0.1	6.2
	17	8:55	12:05	190	24	1.0	7.6
	18	8:55	11:10	135	16	1.0	7.1
	May 9	9:00	11:30	150	21	1.0	8.4
	1946						
Khan, Mohd. A. R.	Jan. 2	22:45	23:25	40	15	0.7	22.5
Begumpet, India	4				9		
G.C.T.	5	20:55	21:30	35	12	0.7	20.7
	7	3:55	4:50	55	20	0.7	21.7
	8	21:15	22:00	45	10	0.7	13.3
	11	23:30	24:00	30	10	0.6	20.0
	14	22:55	24:00	55	14	0.6	15.2
	15	23:25	24:00	35	7	0.6	12.1
	29	21:25	22:30	65	19	0.8	17.6
	Feb. 2	22:30	23:15	45	11	0.8	14.7
	6	22:30	23:00	30	8	0.8	16.0
	11	21:30	22:30	60	7	0.6	7.0
	12	22:30	23:20	50	10	0.6	12.0
	26	23:15	23:50	35	8	0.6	13.8
	28	20:40	21:20	40	6	0.6	9.0
	Mar. 3	20:00	21:00	60	6	0.8	5.0
	6	21:00	21:55	55	12	0.6	13.0
	26	19:30	20:00	30	4	...	8.0
	30	21:30	22:30	60	8	0.6	8.0
	Apr. 1	22:00	22:40	40	5	0.8	7.5
	26	20:30	21:30	60	10	0.7	10.0
	May 1	21:30	22:30	60	10	0.6	10.0

Observer and Station	Date	Began	Ended	Min.	Met.	F	Rate Unc.
Khan, Mohd. A. R.	May 3	21:45	22:30	45	11	0.7	14.7
	5	23:15	23:25	70	21	0.6	18.0
	10	22:00	22:45	45	6	0.6	8.0
	June 4	19:25	19:50	25	4	0.5	9.5
	22	17:45	18:30	45	6	0.6	9.0
	30	19:00	19:30	30	8	0.6	16.0
	Aug. 29	22:00	23:30	30	7	0.6	14.0
	Sept. 22	22:45	23:00	15	9	0.6	36.0 (?)
	30	18:30	19:05	35	6	0.6	10.3
	Oct. 4	23:15	23:55	40	14	0.7	21.0
	19	22:00	22:40	40	11	0.5	16.5
	20	23:05	23:50	45	12	0.4	16.0
	21	20:30	21:30	60	28	0.5	28.0
	22	21:50	22:55	65	26	0.6	24.1
	23	23:15	23:40	25	12	0.6	28.6
	27	20:55	22:00	65	16	0.6	14.8
	Nov. 4	22:45	23:25	40	7	0.6	10.5
	22	22:45	23:30	45	9	0.6	12.0
	23	20:45	21:30	45	9	0.4	12.0
	25	21:10	21:55	45	9	0.7	12.0
	{ 29	20:00	20:55	55	0	0.6	0.0
	{ 29	20:55	21:25	30	7	0.6	14.0
	30	22:30	23:00	30	6	0.6	12.0
	Dec. 2	21:30	22:30	60	21	0.6	21.0
	12	23:35	24:00	25	5	0.4	14.3
	13	22:45	23:45	60	18	0.6	18.0
	15	21:00	21:45	45	8	0.5	10.7
	17	20:00	21:00	60	9	0.6	9.0
	19	21:15	22:15	60	15	0.6	15.0
	20	20:55	22:00	65	12	0.6	11.1
	21	23:15	24:00	30	11	0.6	22.0
	23	21:30	22:30	60	21	0.7	21.0
	27	20:00	20:30	30	5	0.5	10.0
	30	20:30	21:15	45	10	0.6	13.3

Flower Observatory, Upper Darby, Pa., 1947 May 16.

Contributions of The Meteoritical Society

(Known Formerly as The Society for Research on Meteorites)

Edited by FREDERICK C. LEONARD

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A Report on Dust Collections Made at Mount Weather and Arlington, Virginia, 1 October to 20 November, 1946

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ABSTRACT

Procedures employed in the collection of atmospheric dust during the interval 1 October to 20 November, 1946, are described. Details are given on the microscope technique used in the examination of the dust collected. From an analysis

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of the results obtained, arguments are adduced favoring the hypothesis that certain of the dust particles collected were of cosmic origin.

§1. *Location and Technique of Collection.*—From 1 October to 16 November, 1946, ordinary microscope slides were exposed at Mount Weather, Virginia, on top of the Administration Building of the old Weather Bureau Observatory.† The slides were covered with glycerin, and a surface of 2 square inches was evaluated under the microscope. The slides were changed every 24 hours. The slides were sticky enough so that in light rain the dust was not washed off. Mount Weather is about 40 miles from Washington, D. C., far from any source of pollution, at the crest of the Blue Ridge Mountains. At Arlington, Virginia, 7 miles from Washington, D. C., in a residential district, a dust-collecting cup of 7 square inches' surface was used. Rain collected in the receiver with the dust. The rain water was filtrated, and the residue of dust was transferred to microscope slides. The dust there was collected from the cup after each rainfall.

§2. *Microscope Technique.*—The slides with the dust were examined under the microscope for the presence of magnetic dust. The dust was immersed in glycerin in all cases. On top of the stage, 2 alnico magnets were placed with opposite poles 1 mm. apart. With a magnification of 80×, the edges of the magnets were just barely visible in the field of the microscope. The slides were moved over these 2 magnets during the inspection. The non-magnetic dust would move with the slides and disappear from the field of vision. Magnetic particles would, however, stay in the field. In that fashion the number of magnetic particles on the slide surface could be counted, their size observed, and their shape noted. The technique was first tested with some iron filings on a slide and was proved to be very successful.

§3. *Results of the Observations.*—The number, size, and character of the magnetic particles observed are given in the following table.

TABLE 1

(*a*) Mount Weather Exposure

Date	*No. of Magnetic Particles*	*Size & Shape of Particles*
15 Oct., 1946	3	0.02 mm., round, opaque; 0.03 mm., round, opaque; 0.04 × 0.005 mm., wedged-shaped, opaque
16 Oct., 1946	1	0.04 mm., irregular, weakly magnetic rock aggregate, brownish, translucent
17 Oct., 1946	1	0.04 × 0.005 mm., wedged-shaped, opaque
24 Oct., 1946	4	0.1 × 0.04 mm., irregular rock particle with translucent material surrounding a black core; 3 spherical particles, 0.005 to 0.01 mm. in diameter
3 Nov., 1946	1	0.06 mm. in diameter, translucent, irregular-shaped rock particle
6 Nov., 1946	2	0.1 × 0.04 mm., brown rock particle with black portion; 1 spherical particle, 0.005 mm. in diameter, opaque
7 Nov., 1946	1	0.02 × 0.005 mm., rectangular, opaque
14 Nov., 1946	2	0.02 mm., round particles, opaque

†The coöperation of Mr. Wing B. Agnew, Supervising Engineer of the U. S. Bureau of Mines at Mount Weather, and Mr. Fowler, Caretaker of the Observatory, who exposed the slides, is gratefully acknowledged.

(*b*) ARLINGTON EXPOSURE

Date	*No. of Magnetic Particles*	*Size & Shape of Particles*
Interval of Collection		
11 Oct. -13 Oct., 1946	1	0.06 mm., round, opaque
13 Oct. -18 Oct., 1946	3	0.02 mm., round, opaque; 0.03 × 0.005 mm., wedge-shaped, opaque; 0.04 × 0.03 mm., translucent rock particle
29 Oct. -7 Nov., 1946	6	3 particles, 0.02 mm., round, opaque; 1 particle, 0.005 mm., round, opaque; 2 particles, 0.02 mm., irregular-shaped rocks, translucent

§4. *Discussion.*—The purpose of collecting this dust was to find out whether, after the Giacobinid meteoric shower of 8-10 October, 1946, any particles of cosmic dust fell to the Earth's surface. There is, at present, no method that will permit us to distinguish conclusively between dust of cosmic and terrestrial origin collected in the previously outlined fashion; yet certain indirect reasoning can be used to explain the observations.

In the first place, it is unlikely that the large, irregular, slightly magnetic, and translucent rock particles are of extra-terrestrial origin. They are probably dust of local origin with inclosures of magnetite. Such dust is common in the vicinity of the localities where the observations were made. This reasoning leaves as suspicious particles, the globular and the wedge-shaped ones, both of which were strongly magnetic and completely opaque. The larger spherical particles were similar to those described as cosmic dust from the deep-sea sediments collected by the Challenger Expedition, altho these latter particles were somewhat larger in size. The wedge-shaped spicules might possibly be streamlined fragments of larger meteoritic material.

It is noteworthy that, in the week before the Giacobinid shower, *no* magnetic particles were noted at either Mount Weather or Arlington. The first appearance of magnetic dust at Arlington was in the sample collected between the 11th and the 13th of October, and at Mount Weather, in the sample of 15 October. The Arlington particle was pretty large, and had an estimated rate of fall of 100 miles per day. The Mount Weather particles had an estimated rate of fall of 13 miles per day.

Radar contacts with the meteoric shower established the height as 45 to 180 miles from the surface of the Earth. This fact would mean that the particle caught at Arlington would have been at that height 1 to 2 days before reaching the surface. If it is assumed that it entered the collector on the 11th of October, it might well have been a piece of dust associated with the shower. The particles caught at Mount Weather, if they originated in the meteoric shower, would have traveled 5 to 7 days thru the Earth's atmosphere. This circumstance would place their original height at 60 to 90 miles, which is of the same order of magnitude as that of the radar contacts. The same conclusion would apply to the 3 particles caught between the 13th and 18th of October at Arlington.

The 3 small, spherical particles caught on 24 October, 1946, at Mount Weather also might be attributable to the shower. Their rate of fall was estimated at 3½ miles per day. This rate would have placed them, 14 to 16 days before sedimentation on the slide, at 50 to 60 miles above the Earth's surface.

There is, of course, no absolute proof at the moment that these particles were cosmic dust associated with the Giacobinid shower, yet the fact that no magnetic dust appeared in the collections from 1 to 11 October, 1946, or from the 8th to

the 20th of November, except the 2 particles caught at Mount Weather on the 14th, shortly after the date at which Leonid showers have occurred in earlier years, may be adduced as an argument in favor of the hypothesis that these particles were of cosmic origin.

Next to the development of a reliable technique to distinguish between magnetic particles of terrestrial and extraterrestrial origin, it would be highly desirable if systematic collections of cosmic dust were made. Collections should be made preferably at high-altitude stations, in order to eliminate most of the terrestrial sources of magnetic dust. Daily change of the surfaces exposed for the collection also is to be advocated, because that would show variations better than cumulative collection over longer periods of time. It would seem desirable likewise to collect on rather large surfaces, so as to avoid the extreme sampling error that affects collection on microscope slides.

Further Notes on the Puente-Ladron, New Mexico, Aerolite (1068,344)*

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ABSTRACT

Further details concerning observations on the Puente-Ladron, New Mexico, aerolite (1068,344), at the time and place of its discovery, are presented with the conclusion that the stone was found on the site where it had fallen. The probable presence in that area of other stones of the same fall is set forth and an intensive search is again urged.

My recent attempt to give meteorite-hunters a "break," by inviting them to share with me in the search of an area where a new discovery had been made, seems to have resulted in nothing more profitable than an extended argument as to whether there might be something for which to hunt! Apparently some of the present-day meteoriticists choose to solve field questions with a pencil rather than to take the hard way; but, before criticizing those who are skeptical of the results of my suggested search for additional Puente-Ladron aerolites, I must confess to a failure on my part in not setting down all of the details of my observations made on that specimen. The rather lengthy dispute that has now gone into *C.S.R.M.*, **3**, No. 5, 1946, concerns a point on which I made very careful observations at the time of the find. It was my opinion, until about 4 weeks ago, that these observations had gone into my paper. But on re-reading my original paper (*C.S.R.M.*, **3**, 165-7; *P. A.*, **52**, 407-10, 1944), I found that the point had been omitted. Consequently I feel under obligation to report on the matter.

Let me state at the outset that, some 20 years ago, I began checking all of my meteorite discoveries as to any evidence that human agencies may have been responsible for their location where found. At least 2 papers I have written dealt with this matter. One of the first points to receive my attention concerning the Puente-Ladron specimen was whether it had fallen where found or had been transported there. An inspection of the stone made 2 points clear: (1) it had not been much handled; and (2) it had not been exposed to the sand-blasting of wind action. Except for stains, the crust has a completely fresh appearance. The delicate edges and points that characterize the wrinkles and other protuberances

*Read at the Ninth Meeting of the Society, 1946 September.

of a fusion crust appear in Puente-Ladron as they do on a freshly-fallen stone, save for color. The exposed interior, where the crust has been broken away, appears not to have suffered the gnawing action of the desert wind that I have noted in so many of the Pasamonte, New Mexico, stones and in those from several other falls. There was found no evidence of an Indian camp site at the point of find. There *was* such evidence across the river, however, and in several places not farther away than a quarter of a mile, where chipped flint was picked up; but there was none at the point of the meteorite find. The vegetation growing in the vicinity where the stone was found gives evidence of the comparatively constant surface level of the soil. The soil in which the stone was embedded is an alluvial deposit, but one that is not often disturbed by flood waters. As stated in my original paper (*loc. cit., ante*), the specimen shows about the same degree of weathering as did similarly situated stones from the Holbrook, Arizona, fall, found 19 years after their arrival on the Earth. Climatic conditions and soil conditions are similar at Puente-Ladron and Holbrook.

I should like again to urge that this region (Puente-Ladron) is a likely place in which to search for aerolites. None of the objections that have been raised will dissuade an experienced field man from the recognition of its possibilities. Far more effort went into a search of the Gladstone, New Mexico, area than has gone into the Puente-Ladron location, before a single find was made; yet the American Meteorite Laboratory discovered 4 different meteoritic falls (aside from Pasamonte), aggregating about 280 pounds and some 40 stones, all within a radius of 5 miles, in the Gladstone vicinity—an area not too large to be encompassed by a single shower.

I have spent several hours searching in the Puente-Ladron area with no visible results, but until at least 80 days of searching by various persons have been reported, I shall regard this as prime territory for the finding of meteorites. If 10 stones from this fall are visible on the suface, to the same extent that stone No. 1 was visible, and if 80 days of searching are spent, I would say that the chances are 3 to 1 against any of those stones being found. The likelihood is, however, that 100 or more stones are scattered over an area of 10 square miles. Some of these should be much larger than stone No. 1, and, if any of them are lying exposed, a thoro search may result in their recovery. Searching for meteorites requires a lot of patience, but the least fruitful occupation is that of figuring out reasons why *none* can be found! It is a fair conjecture that, among those who are not well acquainted with the Barringer (Canyon Diablo, Arizona) Crater area and are not experienced in field work, more hunting time has been spent between finds of metallic specimens there than has been spent by all concerned in searching for Puente-Ladron specimens.

Final Remarks on the Puente-Ladron, New Mexico, Aerolite (1068,344)

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ABSTRACT

This note consists of comments on the preceding paper. The conclusion is reached that meteoriticists might much more profitably spend their time in searching in the hospitable and inspiring neighborhood of the Canyon Diablo, Arizona, Crater for additional fragments of the shower of which the 2,789-gram Coon

Butte, Arizona, aerolite was a member, than in hunting for companions to the tiny 7.7-gram Puente-Ladron, New Mexico, aerolite, in a hypothetical strewn field situated in a region which outranks even Arntz, Arizona, as a meteoriticists' purgatory.

Thru the courtesy of the Editor, and as a member of the Society's Committee on Publications, I have been given an opportunity to read the preceding paper, "Further Notes on the Puente-Ladron, New Mexico, Aerolite (1068,344)" (hereafter to be referred to as *N*), prior to its publication. The numbered comments that follow relate to statements made in the corresponding paragraphs in *N*.

(1) The author of *N* has developed many meteorite-strewn areas where the weight of the initial find and other circumstances effectively ruled out the harassing possibility that this find was transported by humans to the point of discovery. If, as he asserts, he desired to give meteorite-hunters a "break," why has he not published as exact and helpful instructions for reaching these promising fall areas as he has given for the unpropitious Puente-Ladron fall?

(2) The author of *N* has presented no facts either disproving the statements made or discrediting the conclusions drawn in my two earlier notes on Puente-Ladron.[1] The exiguous Puente-Ladron stone may have been carried by an Indian into the area where it was found in 1944, and there soon discarded or lost, under circumstances permitting it to remain buried most of the intervening time, until it was at last discovered while temporarily laid bare by wind scour. Nothing in *N* rules out this evident possibility—certainly not the evidence of vegetation. As stated by the discoverer of Puente-Ladron in his original article,[3] the terrane where the aerolite was found "is almost bare of vegetation." The extremely scanty vegetation does not give proof of "the comparatively constant surface level of the soil" over any larger an area than is covered by the vegetation itself, and the Puente-Ladron aerolite was *not* found in a clump of vegetation! Certainly not failure to discover *surface* evidence of an Indian camp site at the point of find does so. Excavation there might tell a different story, and, in any event, I have never insisted that the aerolite was necessarily "discarded or lost" *in* an Indian camp. Certainly not microchemical or microscopic analyses of the Puente-Ladron stone itself do, for nothing has been published proving that it is really a new fall, and thereby precluding the possibility that the 7.7-gram specimen was picked up by an Indian in any one of several meteorite-strewn fields now well known and within very moderate distances of the Puente-Ladron region.

(3) Accepting the evidence as to the condition of the surface of Puente-Ladron, tardily presented in *N*, we are still faced with a "meteoritical position problem"[2] admitting at least two solutions. In my opinion, the solution suggested in my earlier notes is the most probable one. Furthermore, meteorite hunting in the Puente-Ladron neighborhood is subject to even worse handicaps than in the area covered by the Holbrook, Arizona, shower, at Arntz. Consequently, I cannot agree that the Puente-Ladron region "is a likely place in which to search for aerolites."

(4) It is impossible for me to accept the statement that "more hunting time has been spent between finds of metallic specimens" at the Barringer (Canyon Diablo, Arizona) Crater "than has been spent by all concerned in searching for Puente-Ladron specimens." On many occasions I have been with groups searching visually for metallic meteorites in the neighborhood of the Barringer

Crater. My records show that in such searches experienced observers pick up metallic meteorites, as heavy as the Puente-Ladron specimen, or heavier, every few minutes, and that even the most inexperienced hunter makes a find of this sort every hour or so.* Moreover, the Canyon Diablo area can be most strongly recommended to the meteorite-hunter not only because of the grandeur of the Crater and the profusion of small iron meteorites about it but also because every reason that has been advanced as justifying an intensive search of the Puente-Ladron neighborhood for aerolites applies with much greater force to the Canyon Diablo region itself. In contrast to the 7.7-gram Puente-Ladron specimen, the initial aerolitic find at Canyon Diablo (namely "Coon Butte") scaled 6 pounds, and for this "weighty" reason one may disregard the possibility that it was transported to the point of find in a medicine man's pouch! Furthermore, the Coon Butte, Arizona, aerolite, found about one mile west of the Crater by D. M. Barringer and S. J. Holsinger, in 1905, has been so widely distributed and so carefully studied that one need have no hesitation in accepting it as a distinct fall. If ". . . no stony meteorite ever penetrates our atmospheric blanket to reach the soil unbroken,"[3] and if "every fall of stony meteorite is more or less of a shower,"[3] then rich returns await a careful search of the Canyon Diablo area for *aerolites!* Furthermore, in view of the considerable weight (2,789 grams) of the initial Coon Butte find, this find must have been placed much nearer the center of the strewn field than was the tiny Puente-Ladron stone (7.7 grams); consequently, the size of the area to be searched for additional Coon Butte specimens is doubtless much smaller than is the corresponding area at Puente-Ladron.

In conclusion, I wish to point out that the Puente-Ladron stone has long enjoyed the dubious distinction of being the least examined of meteorites of uncertain identity. With the appearance of *N* and the present critique, Puente-Ladron now achieves a second questionable distinction, *viz.*, that it is the meteorite with the most published words per gram! It is my opinion that discussion of Puente-Ladron should cease until someone finds additional specimens of the fall large enough to permit exhaustive study and identification.

REFERENCES

[1] La Paz, Lincoln, *C.S.R.M.*, **3**, 243-4 and 271-3; *P. A.*, **54**, 95-6 and 367-70, 1946.
[2] La Paz, Lincoln, *C.S.R.M.*, **3**, 148-53; *P. A.*, **52**, 300-6, 1944.
[3] Nininger, H. H., *C.S.R.M.*, **3**, 165-7; *P. A.*, **52**, 407-10, 1944.

[The foregoing are the fifth and sixth—and they shall be the last—papers to be published in these CONTRIBUTIONS on the subject of whether the 7.7-gram Puente-Ladron aerolite fell where it was found or was transported thither by man.—ED.]

Death of V. M. Goldschmidt

V. M. Goldschmidt, 59, Director, Geological Museum, University of Oslo, Norway, died March 20. After several periods of imprisonment during the occupation of Norway, Dr. Goldschmidt escaped early in 1943 to Sweden and then to Scotland and England. He worked in the MacCauley Institute for Soil Re-

*Beginners usually fail, because they spend their time in searching on the rim and slopes of the Crater itself rather than at distances of one-half mile or more away from the Crater rim, particularly in the west and westnorthwest sectors.

search, Scotland, and Rothamstead Experimental Station in England during the remainder of the war.—*Science*, **105**, 358, April 4, 1947.

Dr. Goldschmidt was one of the most distinguished Fellows of the Meteoritical Society. His death is recorded with a deep sense of loss, both to science and to this organization.

A Student's Answer to "What is a Meteor?"

In answer to the question, "What is a meteor?," on a recent examination in elementary astronomy, at the University of California, Los Angeles, a student wrote as follows:

"A meteor is the flash of light
Made by a falling meteorite
When rushing thru the air in flight,
And usually is seen at night.
(I hope to heav'n this answer's right!)"

It was, even to the *meter* (no pun intended!), and he received full credit for it!

New Meteorite Craters in Eastern Siberia Reported

Under the title "Fallen Planet?" the following account appeared in the magazine *Time* for May 5, 1947 (p. 74):

"Astronomers thruout most of the world got that frustrated feeling last week over a bit of news that plodded, with maddening deliberation, out of Russia. According to V. G. Fesenkov, Chairman of the Meteorite Committee of the U.S.S.R. Academy of Sciences, a 'minor planet' landed on eastern Siberia last February 12. The fragments of iron, nickel, and cobalt were said to have smashed thru the soil, penetrated the bedrock, and left several dozen craters—the biggest one 75 feet in diameter.

"What hit Siberia was probably a wandering meteorite, rather than a 'minor planet' belonging to the Sun's very orderly family. If it had fallen anywhere else, the world's astronomers would have pounced on it before the crater was cold. They could only hope that Soviet scientists were making accurate, if tardy, observations."

A similar, "A.P." report from Moscow, dated April 28, was published in *The Honolulu Star-Bulletin* of that same date. To date of May 7, no further information on this subject has been received.

Appointment of Dr. C. C. Wylie as the Director of the Meteor Section

An extension of the field of work covered by the Meteoritical Society was voted by the adoption, at the Ninth Meeting, in September, 1946, of a new Article of the Constitution of the Society. This provides for the formation of a Meteor Section, thru which our Society, working in close coöperation with the American Meteor Society, may extend facilities to those of its members who are interested in research on meteors, for coördinated and directed work in that field. This new Article 5, entitled "Meteor Section," reads as follows (*C.S.R.M.*, **3**, 291-2; *P. A.*, **54**, 480-2, 1946):

"Sect. 1. The Meteor Section of the Society shall specialize in the observa-

tion and study of meteors; it shall be open to every member of the Society in good standing who is interested in doing that kind of work; and it shall consist of all such members who have joined it.

"Sect. 2. The Meteor Section shall be under the management of the Director of the Meteor Section, who must be a member of the Section."

Since the office of the Director of the Meteor Section (*cf.* also Art. 3, Sect. 1, of the Constitution, as amended at the Ninth Meeting) was created by Art. 3, Sect. 1, and Art. 5, Sect. 2 (just quoted), that office could not be filled by the election that was held in connection with the Ninth Meeting; hence it has devolved upon the Council to fill the office by appointment, for the remainder of the 1946-50 term.

Under date of 1947 May 8, the Council has unanimously named Dr. C. C. Wylie, Professor of Astronomy in the State University of Iowa, Iowa City, as the first Director of the Meteor Section. Dr. Wylie is well known for his work in meteoritics and meteoric astronomy; he served as a Vice-President of the Society during its first two terms (1933-41); and he is, and has been for some years past, the Secretary of Section D (Astronomy) of the American Association for the Advancement of Science, of which our Society is an affiliated organization. He is, then, unusually well qualified to undertake the duties of the Director of the Meteor Section.

In accordance with Art. 5, Sect. 1, of the Constitution, every member of the Society who is interested in the observation and study of meteors is cordially invited to communicate with Dr. Wylie and to join the Meteor Section, so that the activities of the Section may get under way.

ARTHUR S. KING, *President of the Society*

1947 May 8

President of the Society: ARTHUR S. KING, Mount Wilson Observatory, Pasadena 4, California

Secretary of the Society: OSCAR E. MONNIG, 1010 Morningside Drive, Fort Worth 3, Texas

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VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Variable Stars in Harvard Bulletin 918: The first post-war Harvard Bulletin is devoted almost exclusively to reports on variable stars. Some of these have been referred to from time to time in these Notes, especially those pertaining to the novae which have been in the limelight in recent years.

Nova Aquilae 1945, discovered by Dr. Nils Tamm in August of that year, has been studied by Anne Hagopian and Constance B. Sawyer from plates taken at Harvard and Smith College Observatories. The plates cover the period from August 29 to November 28, 1943, including four pre-discovery observations, one on August 26, magnitude 6.8, and three on August 27, magnitude 7.4.

The nova was also photographed on photo-red plates, thus permitting the

derivation of color index. The color index increased as the nova became fainter. As the investigators remark, color curves of novae have little physical significance, for they represent mainly the normal sequence of spectral changes of any nova. The possibility that a seventeenth magnitude star found near the position of the nova on plates taken over the years 1911 to 1937 may be the nova, is rather discounted.

Visual observations of Nova (T) Coronae Borealis as made by Joseph Ashbrook during the 1946 outburst are listed, and agree closely with the photometric curve observed by Pettit of Mount Wilson Observatory. The behavior of the nova during the 1946 outburst follows remarkably close the activity of the star during the 1866 outburst, indicating that the agreement is not accidental, but serves as a clue to the mechanism of the outburst. The star was also observed on Harvard plates from February to May, 1946, by Miss Frances W. Wright, and the magnitudes are listed for 24 nights during this interval of time.

Photographic observations of RS Ophiuchi, recurrent nova of 1898 and 1933, by S. Gaposchkin, are listed from April, 1933, to July, 1946, during the minimum stages of the star's activity. The nova was photographed on 62 nights, and results confirm the light curve derived from visual observations made over these same years, in that the brightness at minimum varies in an irregular manner to the extent of at least one magnitude.

The so-called "iron" star, XX Ophiuchi, has, as the result of an inquiry from Dr. P. W. Merrill, been extensively studied from Harvard plates by Dr. S. Gaposchkin. XX Ophiuchi is an irregular variable of small range, with a Bp spectrum, consisting primarily of bright lines, but differing greatly at different times, according to Merrill. Although the light curve might suggest that the star is of the R Coronae Borealis type, with a normal maximum brightness, becoming fainter at irregular intervals, in no other respect does it present the characteristics of R Coronae Borealis. It probably is unique in its spectroscopic behavior. The photographic range in brightness is from about 9.2 to 10.7, and during the years between 1931 and 1946 the star remained at practically constant maximum magnitude. The two minima in 1931 and 1946 might suggest the variations of an eclipsing star, but the behavior previous to 1931, as discussed by Prager, seems to preclude that possibility.

The semi-regular variable Rho Cassiopeiae is discussed by Mrs. Gaposchkin and Mrs. Mayall, both spectroscopically and photometrically. For many years the light variations, as observed both in photographic and visual light, followed a very irregular pattern, varying between magnitudes 5.5 and 7.7 photographically, and 4.5 and 6.2 visually, with no indication of periodicity. The decrease in light during 1946 was decidedly more marked in photographic light than in visual light. The spectrum is distinctly variable, varying from F8 through G to M5p, tending to be of latest type when the star is faintest.

AS Eridani, an eclipsing star of considerable interest, is discussed by S. Gaposchkin both spectroscopically and photometrically. The spectrograms were secured by Gaposchkin while a guest of the McDonald Observatory in December, 1944. The spectrum is that of a typical A0 star of the main sequence. Of the many spectrograms secured, one fell almost in the middle of the eclipse, with the spectrum remaining practically the same as at maximum brightness. Radial velocity measures indicate a range of from 20 to 60 km/sec; not a large amplitude.

Some 500 estimations of brightness were made, and the period of $2^d.66415$ cited in Schneller's 1941 Catalogue seems to require no correction. It is assumed that the eclipse is total, the light curve exhibiting a constant brightness at maximum, with no secondary minimum. The minimal brightness is assumed as 9.00, with a range in variation of 0^m66. The properties of the system are normal for an Algol-type star, including period, range, rotation, radii, inclination, and spectrum, and during minimum the eclipsed component is the one whose spectrum is visible. Puzzling features of the system are, first, the fact that the spectroscopically unobserved component is photographically the brighter, while the predicted spectrum of that component should be well seen in the photographic region of the spectrograms. The second puzzling fact is that where a secondary minimum of 0^m2 depth is predicted none is observed. It may be that the eclipse, instead of being total, is really annular. Under certain assumptions, the A0 component may have a mass of about 4 ⊙, and the mass ratio of the A0 component to the F5 one is about 10 to 1; a value not as yet observed directly for any known eclipsing variable. We may have here a rare case in which both components are on the main sequence in every respect, including luminosity, mass, and radius.

Dr. S. Gaposchkin has detected a variation in BD—10°863 (HV 11087) which presents some surprising results. He finds that during the 56 years covered by the observations, only one isolated decrease in brightness occurred, that of the winter of 1943-44. Up to now the longest period suggested for an eclipsing variable is 40 years for S Doradus.

The minimum of this newly discovered variable continued for, at most, 321 days, and the decrease to minimum, 25 days. The photographic range is 0^m8, from 8.5 to 9.3, and the spectrum is given as Mb. Two spectrograms secured at McDonald Observatory do not show the expected features of a supergiant star similar to VV Cephei. Outside of eclipse, the spectrum is that of an ordinary M6 giant, without emission. Gaposchkin tentatively regards the star as an eclipsing binary with a period of at least 55 years, possibly longer. The star is not included in Aitken's catalogue of visual binaries.

The orbit of the eclipsing variable TT Hydrae has been computed by Miss Edith F. Reilly. Hertzsprung's period of $6^d.953401$ has been corrected to $6^d.953$-4124, using observations made during the years 1899 to 1939. The spectral classification for the system is A3 and B6. The eclipse is total. Uniform photometric elements computed by Russell's method are as follows:

Maximum 7^m50 pg.
Range (primary) = 1^m83
Range (secondary) = 0^m04
Ratio of radii = 0.44

$a_1 = 0.227$
$b_1 = 0.227$
$a_2 = 0.100$
$b_2 = 0.100$
$\sin^2 \theta' = 0.0972$
$\sin^2 \theta'' = 0.0060$
$i = 84°.2$
$J1/J2 = 22.5$
$L_1 = 0.185$
$L_2 = 0.815$

Observations received during April: A total of 3,238 observations were received from 59 observers, as follows:

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Ahnert, P.	32	92	Kilby	9	9
Albrecht	2	2	Kitley	35	37
Bappu, M. K. V.	30	73	de Kock	81	333
Bicknell	12	45	Liller	36	36
Blunck	12	13	Luft	3	9
Bogard	16	43	Menkes	1	1
Boone	10	10	Oheim	20	20
Brennan	2	2	Oravec	72	205
Buckstaff	12	23	Parks	26	47
Caraioryis	7	14	Peltier	102	130
Chandra	148	166	Raphael	12	28
Chassapis	53	95	Robinson	8	10
Cilley	32	100	Rosebrugh	24	149
Daley	3	3	Segers	25	81
Damkoehler	5	15	Sherman	8	39
Darnell	1	1	Sill	15	15
Davis	5	29	Slemaker	20	40
Elias	60	89	Smith, B. A.	22	222
Escalante	46	57	Swaelen	1	1
Estremadoyro	3	4	Taboada	40	46
Fernald	160	306	Tarbell	5	15
Focas	40	58	Thomas	20	26
Garneau	19	20	Topham	39	49
Greenley	6	6	Weber	29	29
Halbach	40	73	Welker	16	18
Harris	14	14	Welles	9	29
Hartmann	108	111	Whitmarsh	6	13
Hiett	2	2	Zirin	52	58
Howarth	11	11	—		—
Hukill	3	53	59	totals	3,238
Kelly	13	13			

May 15, 1947.

Comet Notes

By K. AA. STRAND

No new comets have been announced since the last monthly report.

COMET 1947 *b* (RONDANINA-BESTER) passed through perihelion on May 20 and has moved far enough north to be followed by observers in this hemisphere. The following ephemeris was given on *H.A.C.* 810 by L. E. Cunningham:

EPHEMERIS, 1947.0

1947 U.T.	α h m	δ ° ′	Mag.
June 4	2 37.8	+10 31	6.3
8	2 44.9	+14 20	6.8
16	3 00.4	+21 10	7.6
24	3 16.8	+27 01	8.4

COMET 1947 *c* (BECVAR) passed through perihelion on May 3 moving south. It will shortly be too near the position of the sun to permit further observations.

The following ephemeris was computed from the orbit given by L. E. Cunningham on *H.A.C.* 807:

EPHEMERIS, 1947.0

1947 U.T.	α h m	δ ° ′	Mag.
June 4	6 03.0	+ 5 32	12.6
8	6 04.0	+ 4 04	12.8

The PERIODIC COMET 1947 *a* (GRIGG-SKJELLERUP) is observable in the morning sky. Dr. Cunningham's ephemeris on *H.A.C.* 806 predicts the following positions:

EPHEMERIS, 1947.0

1947 U.T.	α h m	δ ° ′	Mag.
June 8	22 03.9	+28 30	12.7
16	21 56.0	29 42	13.3
24	46.0	30 18	13.8
July 2	34.0	30 14	14.4
10	20.7	29 28	14.8
18	06.8	27 59	15.3
26	20 53.5	25 51	15.8
Aug. 3	41.7	23 13	16.3

It is seen that the comet is rapidly losing in brightness.

COMET 1946 *k* (BESTER) is still observable in the morning sky. The ephemeris will be found in the April issue.

COMET 1946 *h* (JONES) will be found in the morning sky. Cunningham's ephemeris from *H.A.C.* 792 and 793 is as follows:

EPHEMERIS, 1947.0

1947 U.T.	α h m	δ ° ′	Mag.	
June 8	19 52.7	+43 06	12.2	13.9
16	44.8	44 31	12.4	14.1
24	36.0	45 34	12.5	14.3
July 2	26.6	46 15	12.7	14.5
10	17.1	46 33	12.8	14.7
18	07.9	46 30	13.0	14.9
26	18 59.5	46 07	13.1	15.1
Aug. 3	52.3	45 27	13.3	15.3

The predicted magnitudes were computed with r^4 and r^6 laws, respectively.

The PERIODIC COMET 1925 II (SCHWASSMANN-WACHMANN) with its position in Gemini is getting too close to the sun to permit further observations.

Two other comets in the morning sky are COMET 1946 *a* (TIMMERS) and PERIODIC COMET WHIPPLE. Comet Timmers has an expected magnitude of 19 and Comet Whipple will remain fainter than 16th magnitude through June.

Dr. Paul Herget has discussed the orbit of COMET 1943 *a* (OTERMA) and has found it to be of more than ordinary interest. Its orbit lies at present between the orbits of Mars and Jupiter and is accordingly observable at every opposition like a planet. Preliminary results indicate that it experienced a prolonged close approach to Jupiter in 1937 and will experience another one in 1961. Its present orbit may therefore prove to be only transitory during these 24 years. Accurate observations for the next few years are of special importance for computations of the close approach to Jupiter in 1938. Since the magnitude of the comet during the coming opposition ranges between 17 and 18 the observations will require a larger telescope.

Williams Bay, Wisconsin, 1947 May 12.

General Notes

The Rittenhouse Astronomical Society, of Philadelphia, held its monthly meeting on May 9, 1947, at the Morgan Physics Laboratory of the University of Pennsylvania. Dr. Gustav Land, of the Yale University Observatory, spoke on the topic, "The Satellites of Jupiter."

Midwest Group of Astronomers

The Midwest Group of Astronomers met at the Yerkes Observatory on May 10, 1947. The meeting was very well attended; seventy-five people were present, including amateur astronomers from Milwaukee and Chicago. The papers presented were entitled as follows:

"Six-Color Study of Algol," by O. J. Eggen.

"Photographs with the Tonanzintla Schmidt," by L. E. Erro.

"Photometry of Clusters with the Link Reflector," by James Cuffey.

"A Catalog of Metallic Line A-Stars," by Bidelman, Roman, and Morgan.

"Hydrogen Lines in the Metallic Line A-Stars," by J. L. Greenstein.

"Infrared Measurement of Space-Reddening," by A. E. Whitford.

"The Asteroid Program," by P. Herget.

"Mode of Excitation of Cometary Spectra," by N. T. Bobrovnikoff.

"Infrared Spectra of Planets and Stars," by G. P. Kuiper.

"Molecular Absorption Spectra with Long Paths," by G. Herzberg.

Solar Division Activities

Following is a summary of a report presented by Mr. Neal J. Heines, Chairman of the Solar Division of the A.A.V.S.O., at the annual meeting of the A.A.V.S.O. held at Hood College on May 16 and 17.

SYNOPSIS

Membership	88	
Membership Distribution	21	States, Canada, Peru, Brazil, Australia, Greece, Belgium, Sudan.
Report Blanks Issued	400	
Reports Distributed	234	To U.S. Bureau of Standards, D. C.
Number of Observations	4688	Total to Date 16,628.
Communications Received	636	Total to Date 2,228.
Communications Sent	786	Total to Date 2,751.
Expenses this period	$184.93	All paid by the Solar Division itself.
Balance in S. D. Treasury	$ 57.07	Deposited in 1st Nat'l Bank of Paterson, N. J.

Number of Solar Division Bulletins Issued 7, Copies Distributed 461.
Number of B.S. Reductions Reports Distributed 297.

The observations of the A.A.V.S.O. Solar Division were reduced to a Preliminary Relative Sunspot Number by the United States Bureau of Standards, Dr. J. H. Dellinger, Chief, with Dr. A. G. McNish in charge of this work.

The Preliminary Relative Sunspot Numbers, for the period since the last Solar Division Report to the A.A.V.S.O., given at Harvard College Observatory last October, 1947, and as determined by the United States Bureau of Standards are as follows:

1946		1947	
Sept.	103.9	Jan.	118.1
Oct.	103.7	Feb.	128.4
Nov.	118.3	Mar.	135.5
Dec.	112.5	Apr.	142.1

We are apparently quite near the maximum portion of the present sunspot cycle. It is acertained that the maximum will have really high values. The group of Feb., 1946, was the largest ever photographed. That of March, 1947, one of the five largest ever observed. South Belt Counts remain highest.

We regret to announce the deaths of Mr. H. H. Clayton, Dr. W. L. Holt, and Mr. Knox Lee.

Measurements of sunspot areas and their co-ordinates are made by the Reverend Herbert Kitly, Australia, in a new project.

Dr. Bartlett's project on Granulations and Color in Sunspots has brought to light some very interesting data.

The Migratory Bird project progresses nicely.

We have started a Solar Photography section with Rev. Kearons supervising.

We have added South Africa to our list of foreign countries with an observer at Sudan, in its city Khartoum, Mr. George Caraioryis.

The observations of the Division are now graded to standards.

We have added seven new names of persons interested in solar research and requesting our solar data on a monthly basis.

Book Reviews

Concise Chemical and Technical Dictionary. H. Bennett, editor. xxxix + 1055 pages. (Chemical Publishing Co., Inc., 26 Court Street, Dept. FM, Brooklyn 2, New York, 1947. Price, $10.)

To attempt to cover adequately the ever-growing vocabulary of chemistry and technology by the selection of approximately 50,000 words and trade-names is an ambitious task. It is inevitable that hoped-for definitions will be found missing from any such conveniently sized single volume dictionary. The editor of this volume has maintained good balance between the various fields of modern science and technology. It is a dictionary written for laymen as well as for specialists in any of the fields covered.

The editor has included many new terms and phrases which have come out of the war-time developments in science and technology. Terms are included which were gathered by the Allied Combined Intelligence Objectives in Germany and other occupied countries. However, as may be expected of a volume published so shortly after the release of large numbers of previously secret documents, some terms that should be included have been omitted and terms that have been included are occasionally inaccurately defined or not completely incorporated in the volume. Thus, neptunium and plutonium are incorrectly described; curium, element 96, is included, but americium, element 95, is omitted; and none of these four elements is included in the table of Chemical Elements and Their Discoverers.

Several supplementary sections and tables have been included, which will prove of value to the user. These include the following: the nomenclature of organic chemistry; names and formulae of radicals occurring in organic compounds; the pronunciation of chemical words; the Greek alphabet and definitions

of commonly used Greek symbols; mathematical symbols; apothecary symbols; weights and measures; roughly equivalent measures; scales of hardness; chemical elements and their discoverers; temperature conversion tables; indicators for volumetric analysis; equivalents of Twaddel, Baumé, and specific gravity scales; vitamin values of important foodstuffs; and important organic ring systems.

In the section on the pronunciation of chemical words a list of 435 words is given with their most common pronunciations and an indication of the preferred pronunciation. No indications of pronunciations are given in the body of the dictionary. The omission of correct pronunciations of proper nouns is a failing common to this and other scientific dictionaries.

The concise definitions given in this volume permit averaging fifty definitions per page without necessitating recourse to small type. The typography is excellent.

R. L. SEIFERT.

Carleton College, Northfield, Minnesota.

"Of Me—," by W. J. Humphreys: Washington, D. C. (Printed for private distribution. Pp. 375. $5.00.)

Born in a one-room log cabin on a farm in the State of Virginia is the proud boast of Dr. William Jackson Humphreys, for thirty years professor of meteorological physics at the U. S. Weather Bureau.

The autobiography of 375 pages, published when the author was 85 years old, gives a fascinating story of pioneer life on the farm, but more specially of college and university atmospheres in the south of the date of a half-century ago. Humphreys was closely associated with three universities, Washington and Lee, from which he holds three degrees, the University of Virginia, where he was both a student and later an instructor, and the Johns Hopkins University, Baltimore, then vigorously claiming that it belonged to the South.

The reviewer went to the Hopkins as a graduate student one year after the author started there. Each of us took a Ph.D. degree after three years, his in physics, mine in astrophysics, the researches of both being with the concave grating. For two years, we saw each other almost daily on the top floor of the old Physics Laboratory on Monument Street. Those were great days at the Hopkins. Ames admitted, "that he was, and not too modest to say it, the best teacher of physics in America." Rowland, the great genius in research, and quite an indifferent teacher and never prepared for his lectures, stated on the witness stand in a law suit brought by the Niagara Power Company that he was the foremost electrical engineer in the whole of America.

Looking back now through a half-century and comparing the installation of the 21-foot concave grating on the fifth floor of the Hopkins Laboratory with modern equipment such as one finds in Pasadena, on Mount Wilson, and at M.I.T., one marvels at the superb work turned out in Baltimore. First and foremost was the "Rowland Atlas" of the solar spectrum. An individual spectral line seemed to be a thing of great simplicity, and when its wave length was known with great accuracy, little further information seemed possible. In the middle years of the eighteen-nineties, the wrok of Humphreys and Mohler was a startling revelation in that it demonstrated in forcible manner that the wave lengths of lines in a spectrum were subject to a pressure shift. Mohler investigated the shifts in wave length caused by pressures less than one terrestrial atmosphere while Humphreys

carried his researches with pressures from one to more than one hundred atmospheres. Almost simultaneously, further complications in the structure of a spectral line were announced from Holland by Zeeman.

On account of the fact that the search of both of us at the Hopkins had been with Rowland gratings both Humphreys and the reviewer were invited by the U. S. Naval Observatory to be members of our first two eclipse expeditions, in 1900 to comparatively near-by Griffin, Georgia, and in the following year on a much longer trip, half way around the world to the west coast of Sumatra. Although we were on the same expeditions, we did not work together though each of us was attempting separatively to photograph, by the use of gratings, the spectrum of the sun's atmosphere known as the flash spectrum.

Following the lead of Johns Hopkins in its methods of instruction, a half century ago the American universities were just beginning to adopt the laboratory method of teaching chemistry and physics. At the University of Virginia, Humphreys in 1897 became an instructor in physics. In his book he describes in considerable detail his experiences in starting laboratory work under a professor of the old school who had a distinguished reputation as a class room teacher. Another professor at the University during the author's residence there of eight years was his uncle, Milton W. Humphreys, a stimulating personality with wide knowledge of subjects in addition to his own in Greek.

Earlier in his career, the author had been a student at the University of Virginia. With his degrees of B.A. and C.E. from Washington and Lee University, it was but natural that an attempt should be made to take graduate work at the University of Virginia, where five years earlier the Leander McCormick Observatory had been dedicated. As the reviewer has lived in Charlottesville for a third of a century, he will perhaps be pardoned for quoting from "Of Me—." "I immediately learned that graduate work was not then to the fore at the University of Virginia, save alone in astronomy, in which scholarships were available, so I promptly called on Professor Ormond Stone, the astronomer, at his observatory which housed one of the largest and finest telescopes in the world. He was pleasant but not bubbling over with enthusiasm, holding, at least so I inferred, that astronomy was an exhausted science with all questions answered, save one, and that of little interest—the cause of a very small irregularity in the motion of the moon."

With no openings in astronomy available, he spent a year in physics and chemistry and then for five years he taught at the Miller School located 15 miles from Charlottesville. With his salary of $600 per year, he reimbursed Washington and Lee University for some of his own back tuition and then assisted financially, first one sister and then a second one in acquiring a college education. For the benefit of the young instructors in physics who are seeking positions in these more affluent days, it might be stated that with the degree of doctor of philosophy earned at John Hopkins with a brilliant piece of research, the author in 1897 became instructor in physics at the University of Virginia with a salary of $800 per year, out of which he helped finance his brother through the Medical School in the same institution. (It might be also added that two years later the reviewer became a full-time instructor of astronomy at Columbia University with a salary of $1000 per year).

The author and the reviewer can vie with each other in their experiences of interesting trips hither, thither, and yon on scientific missions. Always a keen

observer, Humphreys describes in lucid fashion with many humorous incidents his trip to Sumatra to observe the 1901 total eclipse. He pokes sly fun at the others of us who were the members of the expedition of thirteen. From Manila, he returned home *via* Japan. He attended two meetings of the British Association for the Advancement of Science. In 1911, the meeting was in Portsmouth, England. Accompanied by Mrs. Humphreys, the trip extended through England and Scotland, then to Holland, Belgium, Germany, Austria, Italy and then to Paris. Those who may plan a trans-Atlantic trip could do no better than turn to the twenty pages that give the highlights of beautiful Europe as it was before the dreadful disasters of World War II. In 1924, the B.A.A.S. met in Toronto. In 1912, Humphreys was one of eight Americans who together with a number of scientists from abroad had a *de luxe* trip organized so that the lucky participants might view the geological marvels of these United States of ours.

After eight happy and productive years at the University of Virginia, the author received most flattering offers from the then Chief of the Weather Bureau, and as the result became Supervising Director of the Research Observatory at Mount Weather, Virginia. Unfortunately, the promised third total eclipse in Spain in 1905 and a year at the Cavendish Laboratory both went with the wind. In reading this portion of Humphrey's book, the reviewer again congratulates himself that he stuck to university life and did not accept a government post more than once offered, otherwise he too might have found that one was forced to do "not what he was efficient at, but what he was told to do." Humphreys has had a singularly happy life in which he has won most of the honors that may come to a scientist including an honorary degree from his Alma Mater, two medals from the Franklin Institute, and election to most of the blue-ribbon academies. He has never been a shrinking violet and has never been afraid to call a spade a spade.

He has been a prolific writer on a subject in which everyone is interested, namely, the weather. His clear expositions and popular style have clarified many problems for the average layman. Next to his research work on the pressure shift of spectral lines, his greatest contribution to science has been his book, "Physics of the Air," of 665 pages, published originally in 1920, with the 2nd and 3rd editions in 1929 and 1940.

The reviewer has enjoyed his friendship for a half century and he found "Of Me—" to be of enthralling interest.

S. A. Mitchell.

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